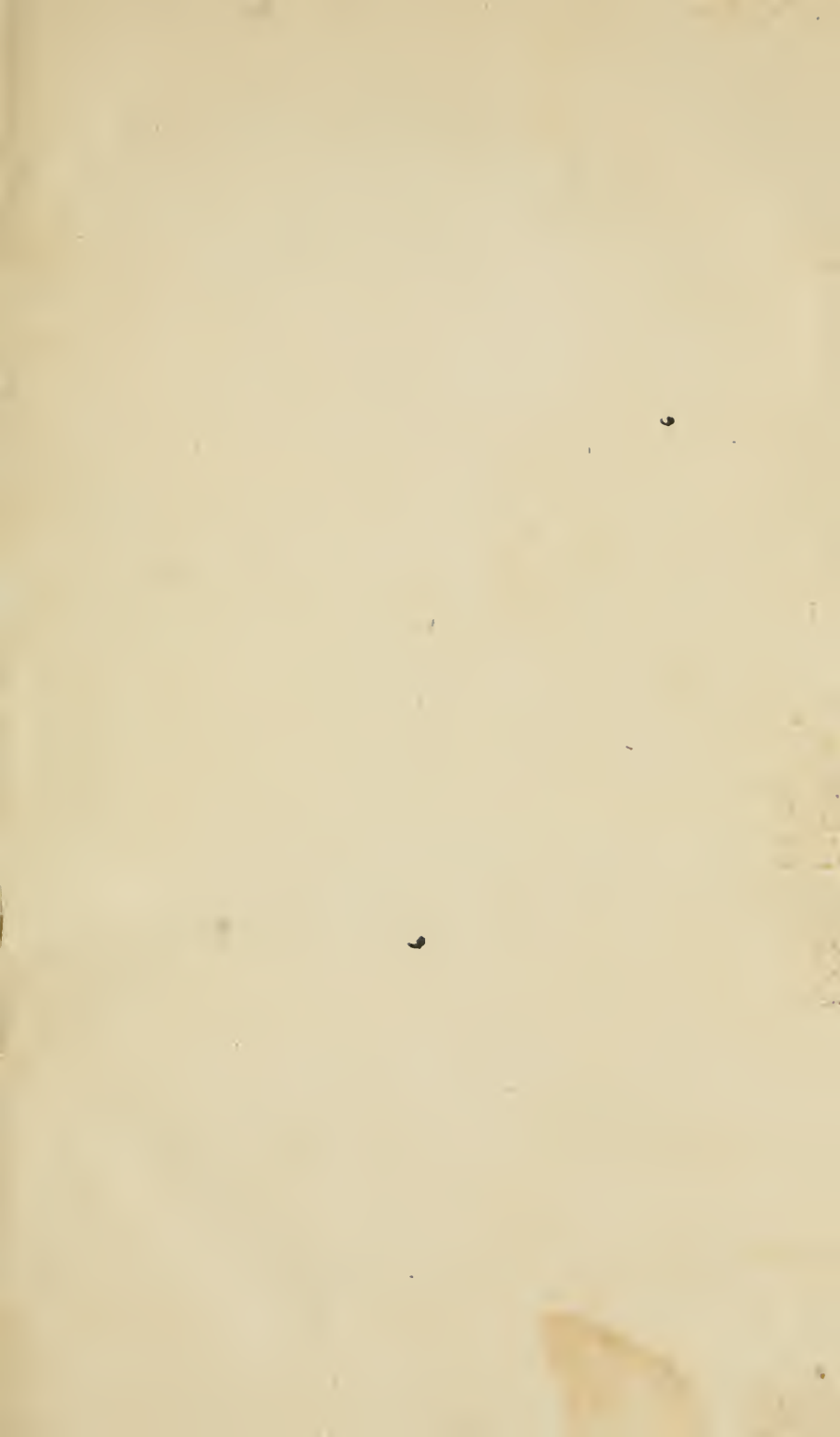


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THE
EDINBURGH
MEDICAL AND SURGICAL
JOURNAL:
EXHIBITING
A CONCISE VIEW
OF THE
LATEST AND MOST IMPORTANT DISCOVERIES
IN
MEDICINE, SURGERY, AND
PHARMACY.

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1827.

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EDINBURGH
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No. XCII.

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Communications have been received from Dr JAMES BLACK of Bolton, and Mr JOHN STEWART, Kelso.

The following publications have been received :—

A Letter to George Renney, Esq. M. D., Director-General of Military Hospitals in Ireland, &c. on the feigned Diseases of Soldiers. By John Cheyne, Esq. Physician-General, pp. 61. 8vo. Dublin, 1826.

A Sermon, occasioned by the death of John Mason Good, M. D., F. R. S., By Charles Jerram, M. A. Vicar of Colham, pp. 64. 8vo. London, 1827.

Organon der Heilkunst; von Samuel Hahnemann, Dritte verbesserte Auflage, pp. 281. 8vo. Dresden, 1824.

Samuelis Hahnemannii Materia Medica Pura; sive Doctrina de medicamentorum viribus in corpore humano sano observatis, e Germanico sermone in Latinum conversa. Conjunctis studiis ediderunt Dr Ernestus Stapf; D. Gull. Gross, et Ern. Georg. a Brunnow, Volumen i. pp. 450. 8vo. Dresde, 1826.

Justification de la nouvelle méthode curative du Dr Hahnemann, nommée Homoeopathie; suivie d'une relation de plusieurs cures conformes à ses principes. Par M. Bigel, D. M. &c. pp. 62. 8vo. Leipsic, 1825.

Traité sur les Effets du Caffé, traduit de l'original Allemand du Docteur et Conseiller Samuel Hahnemann, par G. G. Brunnow, pp. 48, 8vo. Dresde, 1824.

An Oration delivered before the Medico-Botanical Society of London at the commencement of their seventh session, October 1826. By John Frost, F. A. S., F. L. S., &c. &c. pp. 15, 4to. London, 1826.

Clinical Reports of the Medical Cases in the Meath Hospital and County of Dublin Infirmary during the session 1826-27, Part I. By R. J. Graves, M. D. and William Stokes, M. D. Physicians to the Hospital, pp. 83, 8vo. Dublin, 1827. (Printed for distribution among the pupils, not published for sale.)

Strictures on Mr Koecker's Work on the Principles of Dental Surgery, with a few observations on Mr Fay's pretensions, originally published in the Quarterly Medical Review, pp. 14 and 4, 8vo. London, 1826.

Elements of Physics, or Natural Philosophy, General and Medical, explained independently of Technical Mathematics. By N. Arnott, M. D. of the Royal College of Physicians. London, 1827.

An Essay on Gout, &c. &c. &c. By P. P. P. Myddelton, M. D. &c. &c. 4th Edition. Bath, 1827. 8vo, pp. 97.

A Treatise on Clinical Medicine, &c. &c. as contained in the memoranda of J. R. Bischoff, M. D. Imperial Professor of Clinical Medicine, &c. &c. From the German, by Joseph Cope, M. D. London, 1827. 12mo, pp. 280.

A Treatise on the Nature and Cure of Rheumatism; with observations on Rheumatic Neuralgia, &c. &c. By Charles Scudamore, M. D. F. R. S. &c. &c. London, 1827. 8vo. pp. 589.

The Life of Edward Jenner, M. D., LL. D., F. R. S., Physician extraordinary to the King, &c. &c. &c. By John Baron, M. D., F. R. S. London, 1827. 8vo, pp. 624.

Pathological and Practical Observations on Spinal Diseases; illustrated with cases and engravings, &c. &c. By Edward Harrison, M. D. F. R. S. A. Ed. &c. &c. London 1827. 8vo, pp. 294.

Notes on the Epidemic Cholera. By R. H. Kennedy, M. D. &c. &c., Surgeon Bombay Presidency. Calcutta 1827. 8vo, pp. 277.

An Account of the Apparatuses for the treatment of Rheumatism and Diseases of the Skin, which have been constructed at the Dublin Skin Infirmary. By William Wallace, M.R.I.A. &c. &c. 2d edition. Dublin 1827. 4to, pp. 44.

The Anatomy of Drunkenness. By Robert Macnish, Member of the Glasgow Medical Society. Glasgow 1827. 8vo, pp. 56.

Some observations on the Medicinal and Dietetic Properties of Green Tea. By W. Newenham, Esq. &c. London, 1827. 8vo, pp. 32.

Cases illustrative of the Beneficial Effects of the Chlorurets of Oxides of Sodium and Calcium, &c. &c. By J. G. F. Hassel, M. D., &c. &c. London, 1827. 8vo, pp. 31.

A Lecture on the Zopuron, as lately delivered at the Sunderland Infirmary. By William Clanny Reid, M. D., F. R. S. E., &c. Sunderland, 1826. 4to, pp. 14.

A Review of the Diseases of Dutchess County from 1809 to 1825. By Hunting Sherril, M. D. 8vo, New York, 1826.

Observations relative to some defects of the Medical School of Dublin, in a letter addressed to the Board of Trinity College. By John Clendinning, A. M., M. D. 8vo, Dublin, 1827.

Instructions for collecting, preserving, and transporting such specimens of Natural History as appertain to the Animal Kingdom, principally translated from the French. By John Chichester, M. D. 8vo, Cheltenham, 1826. pp. 49.

The Morbid Anatomy of the Human Brain; being illustrations of the most frequent and important diseases to which that viscus is subject. By Robert Hooper, M.D. Bachelor of Physic of the University of Oxford. 4to, London 1826. With 15 plates.

Magazin der ausländischen Literatur der gesammten Heilkunde. Von D. G. H. Gerson und D. Nikol. Heinr. Julius, Januar, Februar, 1827.

Transactions of the Medical and Physical Society of Calcutta. Vol. II. 8vo, Calcutta, 1826.

The New York Medical and Physical Journal. Edited by John B. Beck, M. D. Daniel L. M. Peixotto, M. D. and John Bell, M. D. New York, 1826-1827, Nos. 18, 19, 20.

The Philadelphia Journal of the Medical and Physical Sciences, &c. Edited by N. Chapman, M. D. W. P. Dewees, M. D. and John D. Godman, M. D. No. 7. New Series. Philadelphia, 1826, and No. 8, 1827.

Journal des Progrès des Sciences et Institutions Médicales en Europe, en Amérique, &c. &c. Par une Association de Médecins. I. volume, 1826, pp. 303. IIde volume, 1827. Paris. 8vo, large, pp. 278.

This large and comprehensive Journal, which was commenced in the beginning of 1827, promises to be a useful channel of information to the medical reader. It consists of six parts, 1st, Reviews of Medical works, chiefly foreign, that is German, English, Italian, American, &c.; 2dly, Of a selection of important facts; 3d, A series of monographs, or original communications on subjects in the medical sciences; 4th, Occasional accounts of medical institutions; 5th, A repertory of medical information, alphabetically arranged; and 6th, Necrological and bibliographical annals. In general, the materials are well selected, and in some instances the subjects of the original essays are very important. At some future period we may take an opportunity of speaking more particularly of some of the most interesting.

THE
EDINBURGH
MEDICAL AND SURGICAL JOURNAL.

1. JULY 1827.

PART I.
ORIGINAL COMMUNICATIONS.

ART. I.—*Cases of Ventral Hernia.* By J. H. WISHART,
F. R. S. E. Surgeon to the King in Scotland, &c.

JANET LANE, aged 46, was admitted into the Royal Infirmary on the 28th June 1824. She stated, that, on the morning of the 26th, while carrying a heavy pitcher of water, she was seized with vomiting, which has continued at intervals ever since, with great pain over the whole abdominal region. On the 27th, five tea-cupfuls of blood were drawn from the arm, and some castor oil was given without effect. Four injections were administered in the course of the afternoon without any relief. Four leeches were applied to the tumour, but the pain was increased.

On the right side of the umbilicus there is a hernial tumour pointing to the right side. The cicatrix of the umbilicus is on the left side quite distinct. Several attempts have been made to reduce the tumour. Complains of great pain; has frequent vomiting; countenance anxious; tongue furred; pulse quick and full; no stool since the 25th.

For the last two years she has had a swelling around the umbilicus; but she paid no particular attention to it till the forenoon of the 26th, when she felt severe pain in it.

As it was evident, from the situation and appearance of the tumour, that it was a hernia protruding through a fissure in the immediate vicinity of the umbilicus, and as sufficient time

had been allowed for the different remedies employed, without their producing any benefit, it was resolved to perform an operation without delay, to which the patient readily consented.

The patient being laid on a table in the horizontal position, with her head slightly raised, an incision was made with a convex-edged scalpel the whole length of the tumour, in a longitudinal direction. The integuments were very thin. They were carefully dissected back, which brought the hernial sac into view. A small opening was made into it. The omentum was found adhering firmly to the sac, especially at the upper part. The finger was introduced, and the sac laid open downwards, to the same extent as the external incision. A loop of intestine was discovered occupying the abdominal aperture.

The omentum at the upper part was obliged to be torn with the finger to expose fully the intestine, which was of a dark red colour, its coats were apparently thickened, but retained their natural glistening appearance.

Gentle pressure was made on the protruded gut, which excited borborygmi in the cavity of the abdomen, but did not diminish the size of the protruded portion. The abdominal opening was easily traced around the gut. The point of the finger was with difficulty introduced between the gut and the edge of the opening, and on it, as a director, the stricture was easily divided with a curved probe-pointed bistoury. As soon as this division was effected, and the point of the finger withdrawn, the intestine returned spontaneously.

A large portion of omentum was found filling the sac, some of it recently protruded, part apparently for a long time resident in the tumour, and adhering firmly to the edge of the opening at the upper part. The recently protruded part was easily returned into the abdomen, and part of what remained was cut away with the knife, to allow the lips of the external wound to be brought together. The edges of the wound were secured with two stitches and adhesive straps, and a compress of caddis laid over it, retained by long strips of plaster.

The patient expressed herself as feeling relieved after the operation. She vomited once during its performance.

About an hour after she was laid in bed, an emollient injection was given; and as her pulse was full and strong about twenty ounces of blood were taken from the arm.

She passed a tolerably quiet night; and at six in the morning of the 29th, she began to take every hour an ounce of a solution of the Epsom salts in infusion of roses. Another glyster was attempted to be thrown up, but, as the nurse expressed it, she would not receive it. At the visit she seemed tranquil; the vomiting was less severe; she complained of

severe pain in the left iliac region. Leeches were applied to the pained part; the saline solution was continued; and a purgative injection, with senna and salts, ordered to be administered in the evening. The pain of the left side was relieved by the bleeding; and as on the 30th she had still had no evacuation, the wound was examined. The omentum was of a very dark colour, but there was no gut protruded. The extremities became cold; the countenance collapsed; the pulse very feeble; the vomiting continued, but was less frequent. She was ordered small quantities of wine. She was evidently sinking, and died about 3 P. M.

Dissection.—The portion of omentum which adhered to the neck of the sac, both within and without the abdomen, was in a state of gangrene. The portion immediately adjoining, to the extent of several inches, was much inflamed; but the part in connexion with the stomach, which lay over the great transverse arch of the colon, was of its natural appearance, though much loaded with fat. The portion of intestine which had been engaged in the hernia was a part of the ileum, situated above twelve inches from the place where that gut joins the caput coli.

After its reduction it had regained its natural situation low in the abdomen, and was found adhering, for the space of an inch above the left groin, to the peritoneum, lining the abdominal parietes, and so situated as to brace down and constrict the sigmoid flexure of the colon. At the point of adhesion, and to the extent of a few inches on each side of it, the intestine was gangrenous, so as to yield under the pressure of the fingers, and permit the contents to escape. The ileum and colon were much distended with flatus. The former intestine was slightly inflamed over a considerable portion of its peritoneal coat. The great intestine contained few feces; but the lower part of the ileum, above where it adhered, contained a great quantity of fluid feculent matter.

Case II.—A lady, about 48 years of age, on the 1st August 1826, having felt rather unwell in the morning, took a cup of senna tea; and after breakfast, while in her parlour, without having made any exertion, was suddenly seized with violent spasm in the region of the stomach, to which she had been often liable. On going into her bed-room, and throwing off her clothes suddenly, she observed that a hernia in the vicinity of the umbilicus, with which she had been formerly troubled when in the pregnant state, (seven years having elapsed from her last pregnancy,) had enlarged considerably. She did not, however, suppose that her present symptoms had any connexion with it, and did not at first mention the circumstance. As

the senna tea had not operated, she was directed to take an ounce of castor oil, with forty drops of laudanum, about twelve o'clock.

At 4 P. M. the medicine had not operated ; she had been very sick, and had vomited once.

I was led, from these circumstances, to inquire about the hernia, with which I knew she had been formerly affected, and, on examination, I found a tumour about the size of an egg, of a flattened shape, painful to the touch. I endeavoured, by moderate pressure, to return it, but without success. I then directed a common enema to be thrown up.

10 P. M.—The enema had been returned without any feculent matter. There had been a slight return of the vomiting. The pulse was firm, though of natural frequency ; considerable thirst and dryness of tongue. She was bled to sixteen ounces from the arm. This produced considerable faintishness, and was followed by slight vomiting, and evacuation from the bowels of thin bilious matter.

I again attempted to reduce the tumour, but without success. Cold was applied to the tumour, and effervescent draughts were directed to be given every two hours.

August 2.—Slept a little during the early part of the night ; vomited at three, and at seven a cup of tea about ten minutes after having swallowed it. Ice to be applied to the tumour.

10 A. M.—Has had several thin feculent evacuations, in which some currants were observed, which she had taken the preceding night ; tumour unchanged ; complains of considerable acute pain in it on being pressed ; and a peculiar feeling about the stomach, threatening a return of vomiting and faintishness. Twelve leeches to be applied over the tumour.

4 P. M.—Leeches have bled freely, and she feels relieved ; tumour less painful and not so tense. To apply the ice, and to take a spoonful of a solution of Epsom salts in infusion of roses at intervals. To have some panado, and cooling drinks and fruit.

8 P. M.—Feels much relieved ; tumour less tense, and has rather a doughy feel ; still painful to the touch ; pulse natural ; no vomiting since the morning ; less thirst ; in every respect seems improved.

To continue the saline infusion and the cold to the tumour.

3d.—Symptoms continue favourable ; some headach ; no pain except pressure is made on the tumour ; continue the cold ; *Ol. Ricini* ʒvi.

6th.—Convalescent. Allowed to sit up.

7th.—Attempted to get out of bed, but found she was unable to bear the erect posture. She was recommended to apply the

truss she had formerly worn, but found it excited heaviness and pain in the head. There is still a protrusion as large as the joint of the thumb, not painful, except when pressed on. Bowels quite regular.

From this period the lady gradually returned to her usual state of health. She made various attempts to resume the truss she had previously worn, but found that it invariably excited the uneasy feelings mentioned above. She now wears a concave pad of strong linen, secured by an elastic bandage, surrounding the abdomen. Her general health is good, and she is enabled to follow her usual avocations.

The proper umbilical hernia, or that protruding through the aponeurotic aperture of the umbilicus, is one of very rare occurrence, and is a disease confined to infants. The hernia which has been described in the preceding cases is one of by no means rare occurrence, especially in women in the last stages of pregnancy; and not unfrequently more than one have been observed at the same time protruding in the neighbourhood of the umbilicus.

The ventral hernia, or hernia of the linea alba, very frequently forms so near to the umbilical ring, that it has been supposed by surgeons in general to be a true umbilical hernia. In other cases it appears at a greater or shorter distance above or below the umbilical opening, though it is more frequent above than below, from the linea alba resisting less at the upper part of the abdomen. In those women who have had many children, the superior portion of this aponeurosis is observed to be thinner; and in some women it seems almost wasted away and transparent, and the fibres disposed to separate. A fissure is observed in the linea alba, through which the hernial sac protrudes, containing intestine and omentum, but in most cases omentum only. The hernia of the linea alba is always of a flattened oval shape. The integuments covering it are in general very thin, and hence great caution is requisite in operating in such cases; and it is of considerable importance to distinguish between them and the true umbilical hernia.

The diagnostic characters are thus described by Professor Scarpa: "The umbilical hernia in the infant or adult has always a round neck or pedicle, around which the aponeurotic margin of the umbilical aperture may be felt with the point of the finger. The body of the hernia constantly retains its spherical shape, even when of its greatest size. No puckering of the integuments, similar to what is called the cicatrix of the umbilicus, is perceptible on the upper part or side of the tumour; and there is only at one place a portion of the skin which is observed to be a little paler and thinner than the rest of the

skin covering the tumour. On the contrary, the hernia of the linea alba which forms in the vicinity of the umbilical ring has a neck or pedicle of an oval shape, and the body of the tumour is always of an oval figure. Farther, on examining deeply with the point of the finger around the neck of the hernia of the linea alba, the margin of the fissure of the aponeurosis is felt to be little or not at all elevated; and if it has formed very near to the umbilical ring, the cicatrix of the integuments of the umbilicus is seen still unchanged, and wrinkled, on the one or other side of the tumour, a certain mark that the viscera have not passed out through that opening."

This distinction is of considerable importance in practice.—The hernia of the linea alba, although not restrained by a bandage, increases more slowly than that at the umbilicus. It often passes unobserved for a long time from its small size, especially in fat persons, occasioning colics and uneasiness of stomach, especially after meals, which symptoms are often attributed to a very different cause. The umbilical hernia, however small, and even in infants, becomes instantly perceptible from the change it produces in the cicatrix of the umbilicus, and the rapidity of the progress of the tumour. And, with regard to the treatment, although it be similar in both cases, a cure is much more easily effected in the umbilical hernia, from the natural disposition of the umbilicus to contract, than in that of the linea alba, where this contraction is not so easily excited in the weakened edge of the fissure, through which the contents have protruded.

It has in general been remarked by practical surgeons, that umbilical hernia, and hernia of the linea alba, are more rarely subjected to strangulation than the inguinal or the femoral; but when they do become incarcerated, that they are attended with the most violent symptoms, and that they are more quickly attacked by gangrene than the other species. As a proof of this I may mention that Professor Russell informed me, that, during an attendance of forty years in the Royal Infirmary here, he had never seen the operation performed. In the first case, although the operation was resorted to at a very early period, it was found that gangrene had occurred, and there is every reason to believe it had taken place before the operation was performed. From the recollection of this, I was prepared in the lady, the subject of the second case, to have had recourse to the operation, had the symptoms not yielded during the first twenty-four hours, evidently showing that the intestine had returned, and that the case had been brought to the state of omental hernia only, which, although not without danger, is not attended with the same risk as when the intestine also protrudes. That

latterly omentum only was engaged was evident from the return of the discharge by stool. The vicinity of the strangulation to the stomach is the cause that the consentient symptoms are more violent than in the inguinal or femoral hernia.

In performing the operation great caution is required from the thinness of the integuments, from the omentum generally adhering to the sac, and from the manner in which it usually surrounds the intestine. A longitudinal incision of the integuments is sufficient for exposing the body and neck of the sac. It ought always to be made with great caution, for the reasons already mentioned. In most cases the omentum presents first, unless, in the event of any violent over-exertion, the intestine has perforated the omentum. Considerable difficulty is met with in introducing a probe into the abdomen, from the density of the adhesion; and great caution is necessary until the loop of intestine is exposed. In some cases it may be necessary to cut the omentum with the scalpel or bistoury. In dividing the stricture great care must be employed to avoid wounding the intestine. The stricture is to be divided to the one or other side. The intestine being generally free from adhesion is easily reduced. The omentum, on the contrary, if the hernia has been of long standing, is almost always adherent to the sac, and is almost never reducible. After reducing the intestine, therefore, it will be better to leave the omentum, or to cut away a portion of it, to allow the lips of the external wound to be brought together. In a short time it will be thrown off as the suppuration advances, or its separation may be assisted by means of gentle escharotics.

Edinburgh, February 1827.

ART. II.—*On the Endemic Fevers of Spain.* By T. PROUD-
FOOT, M. D. Kendal.—(Continued from last Volume, p. 260.)

*On the Treatment of the Spanish Intermittent, Remittent,
and Yellow Fevers.*

IT is with considerable diffidence that I venture to detail the best method of treatment. On this subject I shall confine myself to observations of a general nature. The application of general principles to the management of individual cases must always be left to the good sense of the physician. “Nullum ego cognosco remedium, nisi quod tempestivo usu fiat tale,” is a remark that ought constantly to be borne in mind. There is much that can be done for the welfare of a patient, which, if detailed minutely in writing, would appear not only diffuse and

frivolous, but often also unnecessary and ridiculous. By long attendance on the sick, and close attention in conducting complicated processes of cure, a kind of scientific propriety is acquired,—a tact which to the uninitiated may appear intuitive, but which, in reality, is the result of much labour and study.

Yellow fever is so intimately connected with intermittent and remittent fever that it seems necessary, for the sake of illustration, to make previously some general observations on the management of these often very untractable diseases.

At one time bark was considered a specific in the cure of fevers of the periodic type. That delusion has happily gone by; and, although obliged to acknowledge that bark, under certain circumstances, and judicious management, is a remedy of great value, I must at the same time confess, that, on entering on practice with the impressions left on my mind from what I had been taught and had read of its efficacy, I was most grievously disappointed at my want of success. This, however, is not a new observation; for Sydenham complained in a manner somewhat similar, that he knew of no successful method of treating agues, as the bark, which had been regarded as a specific, for the most part only interrupted their progress, and very seldom removed the disease. The inflammatory diathesis was generally so prevalent at the commencement of the Spanish intermittent and remittent fevers, that, to preserve entire the structure of important organs, it was absolutely necessary, before bark could be given with safety, to have recourse to blood-letting, purging, and saline or antimonial diaphoretics, combined with small doses of calomel, according to the circumstances of the case; and so far was this procedure from debilitating the patients, that it often succeeded in effecting a complete cure in a few days. In short, it seldom happens that bark can be given with impunity while irritation in the system is kept up by those local congestions that are so apt to be formed; but once remove them, and the disease, though of long standing, will sometimes spontaneously cease. At all events, then, and not till then, can bark be given to effectuate a radical and complete cure.

The power of the liquor arsenicalis in putting a stop to the disease was also great after a similar preparation of the system. Cordials and stimulants at the onset of the cold stage, at all periods of the disease, were found to be only grateful palliatives, and were very often of questionable propriety at the commencement of an attack. An emetic of sulphate of zinc, or blue vitriol, given before an expected paroxysm, often prevented it; but it was a severe method of treatment, to a repetition of which patients became exceedingly averse.

An extract from my report to the Inspector of Hospitals in

the Mediterranean will show the method pursued at Carthage in the treatment of Yellow Fever ; and although not so minutely detailed as it ought to have been, and although subsequent experience and reading have taught me to consider the propriety of mercurial inunction somewhat questionable, yet I doubt not it will be considered to contain the essence of the best methods that have yet been discovered for treating this most formidable disease.

“ From the appearances upon dissection, and the importance of the organ chiefly concerned, it is obvious that general and topical blood-letting were naturally suggested as the first necessary step. I have taken 120 ounces at four different bleedings in thirty-six hours, and with the happiest effects. After each bleeding the pulse became fuller and softer ; the vomiting, thirst, anxiety, and depression of spirits, gradually gave way ; and the patient almost invariably felt lighter and easier. But the bleeding seemed to be of advantage only when used during the first days of the disease, or perhaps so long as the texture of the stomach had not been destroyed.”

“ After bleeding, the warm bath, putting on clean dry flannels, and laying the patient in a comfortable bed, a calomel purge, assisted by a large enema, the frequent exhibition of a small quantity of some mild diluent drink, frequent sponging of the body with warm water and vinegar to cool the skin, which is often hot, rough, and dry, at the commencement of the disease, and the application of a large blister to the epigastric region, ought never to be omitted. If the symptoms were urgent, and not mitigated by the foregoing measures, and if as large a quantity of blood had already been drawn as from the state of the system seemed admissible, friction with the mercurial ointment, and small but frequently repeated doses of the submuriate of mercury were exhibited, so as gently to move the bowels, and bring on salivation. Here, however, the greatest prudence and discernment are necessary. If the symptoms run high, something extraordinary must be done, otherwise the patient will inevitably perish ; but if time can be gained by bleeding, blistering, the warm bath, &c. the gradual introduction of mercury into the system, so as to obtain its fullest possible specific action, is what ought to be attempted, and will be more likely to insure success than an alterative course. Bleeding, general and topical, blistering, the warm bath, fomentations to the stomach and abdomen, friction with mercurial ointment, and small but frequently repeated doses of the submuriate of mercury, may all be used in urgent cases nearly at the same time, and with the best effects. Should the warm bath be found inconvenient, and the skin remain hot and dry, sponging the bo-

dy with warm water and vinegar, and the internal exhibition of the *Aqua Ammonice Acetata*, will be found beneficial. If the bowels are torpid, the doses of calomel can be increased, or combined with some other cathartic, and *vice versa*."

Blood-letting was resorted to early to moderate increased vascular action, to prevent, if possible, irreparable mischief to important parts suffering unequally in the contest, or to remove general distress, and subvert the morbid process set on foot by the fever. Blood-letting besides acted as a dirigent or precursor to the successful employment of other remedies. Multiplied experience has taught us that the same good effects are not to be expected from blood-letting in idiopathic febrile diseases as in pure and legitimate cases of phrenitis, pneumonia, hepatitis, gastritis, or enteritis. This, however, cannot be urged as a reason against the utility of blood-letting in idiopathic fever, but only cited as a proof that the local affection accompanying fever is of a different and less tractable nature. It is not purely inflammation, but is more nearly allied to it than to any thing else. In the phlegmasiæ the local affection is the cause of the fever :—here the fever is the cause of the inflammatory local affection. Still, even in cases of the latter description, blood-letting, general or local, is known to be the most powerful remedy that can be employed, both for moderating the general inflammatory diathesis, and for removing the local affections.

It is not to be inferred, however, that he who draws off blood most early and copiously will generally succeed, or even be the most successful. Blood-letting in the Spanish fever was commonly most useful when used moderately and discreetly ; the quantity required being from one to two pounds for a full grown person. Sometimes, however, it was used more freely and boldly. In other instances it was used cautiously, and frequently, according to the particular circumstances of the case. In many cases it was quite inadmissible. The system was frequently at the commencement of an attack in a state of depression ; sometimes perhaps from the deleterious effects of the febrile miasmata on the constitution generally ; and sometimes from destructive local congestion, and other causes not at all times well understood. The pulse at the beginning was often in a particularly oppressed state. Sometimes it was too violent ; but frequently it was feeble and irregular.

Atmospherical heat, which is invariably a precursor of Yellow Fever, has a tendency to expand the circulating fluids, while, at the same time, the tone of the heart, arteries, and muscular fibre generally, is relaxed and weakened. Hence persons in comparative health become dull, languid, and feeble ; and the

heart and arteries, relaxed and overwhelmed by a load of blood, become inadequate to propel it. Consequently, on the occurrence of the fever, of which this perhaps is often an ascititious cause, the pulse not unfrequently languishes, labours, and is oppressed. But, by a certain degree of distension, short of the foregoing fulness, the heart and arteries are stimulated to violent and irregular action. Both states are often met with in yellow fever, and, so far as my experience will warrant, blood-letting is eminently serviceable in both. When the action of the heart and arteries is violent, twenty or thirty ounces of blood drawn off quickly, not only diminishes the mass of blood, relaxes and moderates the action of the blood-vessels, renders the pulse softer and slower, cools, soothes, and relaxes the whole system, but also interrupts, and sometimes stops the morbid process altogether; while it tends more powerfully than any thing else to preserve entire the structure, or check farther disorganization in parts of importance to life. When, on the other hand, the system is oppressed by an unusual quantity of blood in the manner stated, the pulse generally becomes full, strong, and frequent after blood-letting; and patients affirm that they feel lighter, easier, and even stronger.

The effects of blood-letting are, however, by no means so simple, or confined to the government or relief of the action of the heart and arteries, but also influence, in a very unequivocal manner, all the operations in the animal machine. A certain degree of tone and vigour of circulation is indispensably necessary to stimulate the brain and nerves to healthy action. The same thing is necessary to maintain the healthy action of the different secretory organs and surfaces, (particularly the skin, which is peculiarly affected in this disease,) the digestive organs, and the due contraction of the muscular fibre, which blood-letting, carried to excess, has a tendency to interrupt. Hence we understand, in addition to many other reasons that might be cited, why blood-letting can only be carried to a certain extent, without detriment to the patient.

The warm bath was a remedy of vast importance in the cure of this fever, and its effects are somewhat analogous to venesection,—allaying heat, pain, or general uneasiness;—relaxing the heart, arteries, and muscular fibre;—soliciting the blood to circulate through the capillaries, and thereby restoring the excretory function of the skin, which, as already stated, was remarkably depraved;—lessening materially the rapidity of the blood through the delicate substance of the brain, and thus relieving the tension of the nervous system;—drawing the blood from the soft internal cellular organs;—equalizing the distribution of heat, as well as the circulation of the blood, which

were both more irregularly allotted in this fever than in most other acute diseases ;—and lastly, most gratefully soothing and acting as a sedative. I have generally drawn off blood first, and subsequently immersed the patient in a warm bath. Sometimes blood was again taken while the patient was in the bath, either when there was violent arterial action, a general feeling of uneasiness, deep-seated racking pains in the back or limbs, or any other symptom indicating an obstinacy of disease likely to be controlled by such a proceeding. Friction with coarse towels or hair-cloth and soap, while the patient was in the bath, so as completely to remove every impurity from the surface of the body, and excite a glow or preternatural degree of sensibility, also appeared eminently serviceable. I have no experience in this disease of the efficacy of salt or *Eau de Cologne* when added to the bath, or of friction with strong stimulating oils when there appeared a deficiency of animation ; but it is not unreasonable to suppose that they may prove somewhat auxiliary. In that depressed state of the system, when the countenance was in an inanimate statue-like condition, the extremities deficient in warmth, and an indifference was shown to external objects, it was customary to immerse the patient in a warm bath first, and to give some warm cordial, such as strong wine-negus, hot water and brandy, or ether and laudanum. In fact, patients were often in such a depressed condition as to render it impossible to procure blood from them in quantity sufficient to make any impression on the disease till a previous process of this kind had been gone through. Stimulants were forced on our notice by necessity. On ordinary occasions they could not be used with impunity.

When there was a determination of blood to the head, the affusion of cold water on the head and shoulders while the patient remained in the bath, was frequently of great service in relieving the headach, and checking the progress of the fever ; but, in accordance with what I have seen, the affusion of cold water over the whole body, on account of the invariable affection of the stomach, and frequent implication of other abdominal viscera, was quite inadmissible. Even the affusion of cold water on the head and shoulders was had recourse to with caution, and was always performed while the patients were in the bath ; for otherwise, by reason of the tendency it had to constrict the vessels on the surface of the body, it annihilated the ordinary good effects of the warm bath.

There are a few collateral indications which ought constantly to be borne in remembrance in the treatment of yellow fever. First, the stomach and brain should be protected from congestion or violent arterial action ; and this indication is to be

accomplished by the ready and decisive measures already mentioned—venesection and the warm bath. The bowels ought next to be emptied by purgatives, the basis of which ought to be calomel in small doses, frequently repeated till the object in view is attained. The next indication is the preservation of the functions of the stomach and skin by a judicious union and well-timed exhibition of cathartic and sudorific medicines. This was most effectually accomplished in the Carthagena fever by combining small doses of antimonial powder with submuriate of mercury, about two or three grains of each given three or four times daily; and occasionally adding to them half a grain of opium, assisted by *Aqua Ammoniacæ Acetata*, or a solution of nitrate of potash, and fomentations or sinapisms to the abdomen. The peristaltic motion of the bowels was thus promoted, and they were kept in the most favourable state to be acted upon, should more efficient purgatives seem necessary. A judicious physician, however, will never confine himself to any particular form of prescription. Individual cases present such phases as to require the exercise of the judgment anew at every visit, and that too exclusive of any material change in the aspect of the disease. Hence, without essentially altering his plan, he will often have to combine and vary the quantity and quality of the medicine, and lengthen or shorten the interval between the doses.

A grain of red sulphuret of mercury added to each dose of the calomel and antimony was sometimes found to render their effects more certain in pervading and searching the system. In mild cases, or when the force of the disease, although at first violent, had been somewhat broken or expended, I have occasionally trusted to milder preparations, such as the mercurial pill, or powder of mercury and chalk, in combination with antimonial powder in small doses, and at longer intervals. In the inflammatory species of this fever Dr Jackson recommends three grains of calomel, three of the genuine James's powder, ten of nitre, twenty of flowers of sulphur, and six of carbonate of potass, to be made into a bolus, and given every four hours with two ounces of infusion of senna, and two or three drachms of *Aqua Ammoniacæ Acetata*. In other varieties of the disease he uses nearly the same remedies, only substituting ten grains of camphor for the nitre, and half a grain or a grain of opium for the potass. In cases of great anguish and irritability at stomach, I should think the bulk of the remedy an objection to its use; otherwise, it seems well calculated to accomplish the object in view. In this, as in other fevers, after a certain state of preparation,—that is, having mitigated arterial action, evacuated the bowels, relieved the anguish in the epigas-

tric region, and rendered the skin perspirable,—I was partial to an anodyne at bed-time. Five grains of calomel, with two or three of the powder of ipecacuanha, and a grain or two of opium, according to the circumstances of the particular case, made into two pills, or calomel with the compound ipecacuan powder, were eminently useful.

(To be concluded in next number.)

ART. III.—*On the Management of Collared Cases in Midwifery, and a safer and easier Method proposed.* By WILLIAM SMITH, Surgeon, of Bideford, and Member of the late Corporation of Surgeons of London.

* * * Si quid novisti rectius istis,
Candidus imperti, si non, his utere mecum.—HOR.

WHEN an individual, long exercised in the several branches of his profession, has made a practical discovery, and finds, after the experience of forty years, that such discovery, when acted upon, is always attended with the most pleasing immediate effects, and, in his opinion, involves future and important consequences; and when he finds, by reference to all the authorities upon his shelves, as well as by communication with his numerous medical friends, that the idea is original, that our best authors record it not, and that it is not adopted by our most scientific practitioners, it becomes his bounden and paramount duty to impart it in the most extensive manner in his power, for the general good.

In order to effect this purpose, I have selected the Edinburgh Medical and Surgical Journal, as it is a work generally perused by that large class of the medical world whose daily employments, in the most laborious parts of our profession, preclude them from more extensive reading, and to whom the subjoined remarks are more particularly addressed.

The subject to which I wish to invite the attention of my readers, is that variety of human parturition in which the child, in popular language, is collared, that is, in which the head is excluded from the vagina, with the funis wound around its neck.

When this occurs, and my medical brethren in obstetric practice well know that it often does occur, it is directed by our teachers in their lectures,* and by various authors on the practice of midwifery,† that, before we allow the labour to pro-

* Louder's Lectures, MS. Hunter on Gravid Uterus, MS.

† Burns' Principles of Midwifery, p. 248. Smellie's Midwifery, Vol. i. Book iii. Sect. 3d. Chapman, p. 126. Edinburgh Practice of Midwifery, p. 195.

ceed to the exclusion of the body of the child, the navel-string should be loosened from the neck and drawn over the head,—or, if that cannot be effected, that it should be divided, and the foetal end secured by ligature.

The first plan, as to its immediate consequences, is certainly the safest and the best; but many circumstances render it often impracticable, and still oftener hazardous. The head may be too much elongated by a tedious labour to admit of its being done without great violence to the cord, or a short funis may render the attempt abortive. In either case, the effort may be followed by a rupture of the cord, premature separation of a portion of the placenta, and consequent hemorrhage, or by a dragging down and displacement of the fundus, which predisposes the mother to future prolapsus uteri. For although the length of the human funis is computed, upon an average, to be two feet, yet it is allowed that it sometimes ranges from six inches to four feet.* I have in my own practice met with variations from ten inches to three feet.†

The narrowness of the *meatus externus*, particularly in first labours, or the violence and rapidity of the last expulsive throes, will often embarrass the accoucheur, and render his endeavours to act upon the first plan above-mentioned ineffectual; and, from one or other of these causes, it frequently happens that the child is projected from the vagina with the cord still twisted around its neck, to the manifest danger of injury to the mother or the child, or perhaps to both.

The second plan recommended by authors, viz. to cut the string, is still more hazardous, although perhaps less difficult. Is the important charge of holding the umbilical end, or securing it by ligature, to be entrusted to a trembling female assistant at the agitating moment of the termination of the labour? Or if the surgeon does it himself, how can he assist his patient by protecting the perineum, and directing the exclusion of the trunk of the child? And be it remembered, that the loss of a small portion of foetal blood, at the moment when nature requires all her energies unimpaired to accomplish the transition from umbilical to pulmonary circulation, if not destructive of the new born infant, may lay the foundation of a future defective or diseased constitution.

If the foregoing remarks, derived from a long course of observation in obstetric practice, be deemed just, permit me to

* Burns' Principles of Midwifery, p. 124. Fife's Anatomy, Vol. ii. p. 325. Hunter on the Gravid Uterus, MS. Mauriceau, *Traité des Maladies des Femmes grosses*, Tom. i. p. 228. Edinburgh Practice of Midwifery, p. 42.

† Smellie on Midwifery, Vol. ii. p. 334.

subjoin my ideas of the more remote pernicious effects which may, and I conceive do, arise from the present mode of managing collared cases.

The immense number of married or breeding females suffering the misery of injured uteri is known only to their medical men. Modesty induces them to conceal their almost hopeless condition from their friends around them. But among our labouring poor, who have become mothers, at least one in twenty will tell you that she has a partial or total prolapsus—a pain in the loins—or some sense of bearing or dragging down, when in the erect posture, or at hard work.

I would ask, then, may not the first plan before described, and usually adopted for the management of collared cases, lay the foundation of this miserable female disorder? And may not the frequency of such cases, and the numerous instances of prolapsus among the poor, be considered as cause and effect, more particularly when it is remembered that their labours are in general unassisted, or, what is worse, left to the violent treatment of ignorant female midwives?

I have no recollection of ever having seen prolapsus uteri in the virgin state; it seems always a consequence of parturition; and if it is unknown in the animal creation, unnatural in the human, and most frequently found among the poorer part of our species, the inference is almost conclusive, that some mischief done during the management of the labour must be its cause.

In the year 1801, I published a case of *inversio uteri* after delivery.* It will appear obvious to the reader that the inversion was created by a very short funis. My notes say the funis was “a few inches in length;” and that, although the reduction was most promptly, and without loss of time completed, yet, three days after labour, a *procidencia* took place, which was again reduced.

This female is still alive. She has had many children since without inversion; but her subsequent labours have always been followed by slight prolapsus, and she now suffers severe pains of the loins and occasional bearing down, although she has passed the female climacteric.

I mention this case in illustration of my opinion, that a shortened funis, whether so in its natural state, or rendered so by the collar, or by the efforts of the midwife to carry it over the head, or by too hasty attempts to extract the placenta,†

* London Medical and Physical Journal, Vol. vi. p. 503.

† White's Treatise on Lying-In Women, Case xix. p. 429.

endangers the displacement of the fundus uteri, and predisposes to future prolapsus.

These remarks will be best appreciated by those of my readers who are extensively engaged in this branch of our profession; and if they agree with me in the conclusions which I have drawn from the premises, they will, by the adoption of the subjoined expedient, co-operate with me in the humane endeavour to lessen the frequency of that peculiar misery, with which the most delicate and most interesting portion of our species is too often afflicted, after undergoing the agonizing pains of parturition.

My plan is as follows:—Having waited the usual progression of the head through the pelvis, until it is excluded from the vagina, if I find that a circumvolution of the funis around the neck of the child exists, I first ascertain which is the umbilical, and which the placental portion of the cord; and this is easily learned, because the foetal portion will invariably be found tight and unyielding, whilst the placental portion will easily yield to a slight effort of extension with the fingers. This being done, I avail myself of as much of the placental portion of the cord as I can obtain, (and I have always found sufficient, when the collar is single,*) gently to extend it to the extreme points of the shoulders. I now wait for the next pain, and when it takes place, I form with the fingers of both my hands an ellipsis of the funis, of sufficient size to admit the escape of the shoulders and trunk through it. The result is obvious and immediate; the labour experiences no interruption; and the final delivery is completed by the foetus passing through the extended noose of the cord, whilst the fundus uteri remains undisturbed, the placenta undetached, and the string unbroken.

This process is so simple that it may often be effected in a minute; and the superior efficacy and safety of my plan over the usual one adopted is proved by the following experiment:

“Having by me a full-grown dried foetus, I measured two feet of a narrow strip of woollen list, being the average length of the

* It is mentioned by authors that those cases of collared, or knotted funes, take place at a very early period of utero-gestation; and where the collar is double, it is obvious that the cord must be unusually long. I have succeeded in extending a double collar over the shoulders, although confessedly with more difficulty. The experiment in the text will prove, that, if it cannot be extended to admit the shoulders, it cannot be brought over the head, and the only alternatives left to the accoucheur, are either to divide the funis, or to suffer the labour to proceed, taking care, if the latter plan be adopted, which perhaps is the best, to keep the trunk of the child as near as possible to the pudendum of the mother, thereby lessening the danger of uterine affections.

funis, also the best substitute I could find, both in pliability and elasticity, for the human cord: I then tied one end to the point of the navel, and having wound it once round the neck of the fœtus, I pinned the other end to the cloth of the table, on which the fœtus lay, at the supposed distance of the placenta. The collar, or circumvolution round the neck, measured five inches; the enlarging of the collar, to bring it over the shoulders, required five inches more; while, to bring it over the head by the occiput, the part usually projected, required nine inches in addition to the length of the collar."

This proves to demonstration the advantage of my system over the former one; for the slipping of the funis over the shoulders, instead of extending it over the head, by requiring four inches less of extension, lessens the danger of displacement of the fundus, partial separation of the placenta, and consequent hemorrhage and flooding, or rupture of the cord.

If it be considered also that this experiment was made upon a dried, unyielding subject, and with a substance far less elastic than the living human funis, it is presumed that the difference between the two modes will be found still more favourable to my plan when tried upon the living fœtus, *in actu parturiente matris*.

If one practical fact be considered of more value than many pages of theory, let my readers make trial of my practice, and they will find that its adoption will prove gratifying to themselves, and beneficial to their patients.

ART. IV.—*On Lithotomy with Mr Key's Knife and Straight Staff.* By ROBERT HULL, Surgeon, Norwich.

I KNOW that some surgeons were struck with the proposition lately advanced by Mr Key, that the opening into the bladder is more easily made by cutting upon a straight staff. They admired the apparent simplicity of the process recommended by this gentleman, the clearness with which he stated his arguments, and the classical character of his whole treatise. But I believe that the majority of operators have not been impressed in favour of this novel plan, because I have neither heard nor read of any operations thus conducted, excepting those performed at Guy's Hospital.

Having formed a favourable opinion of the straight staff in consequence of repeated trials on the dead subject, I used it in the case of a boy fourteen years of age, whom I cut on the 8th of

January last. Having cut into the urethra with Mr Key's knife, received the staff from the assistant, conducted the knife into the bladder, and withdrawn it in the groove, retaining the staff in my left hand, I passed my right forefinger along the groove. I found a sufficient, a complete division of the prostate; and my finger easily entered the bladder. I withdrew the staff; and the forceps, passed with my left hand along the palmar surface of my right fore-finger, entered the bladder just as readily. The stone lay at the neck of the bladder so impacted that I afterwards saw a sulcus formed by the urine on its upper surface, and constituting as it were a commencement of urethra. The calculus lying under the opening in the prostate, the forceps passed beyond it. It remained fixed in its nidus in spite of the curved forceps, the finger and a scoop. Luckily a pair of nasal-polypus forceps were at hand, and with these I readily extracted it, the prostate offering no resistance, although the stone was seized in the long diameter, which measured an inch and one-eighth. It was a rough mulberry calculus, weighing nearly three drachms. On the whole, the operation was rapid, the convalescence satisfactory, and to-day, five weeks after the cutting, he called on me to show that his wound was entirely cicatrized.

The conclusions which I have ventured to draw from this solitary case are the following:

1. The straight staff enters the bladder as readily as the curved; perhaps more readily.

2. The surgeon, holding the straight staff in *his own* hand, keeps the knife more easily in apposition to it, whereby the chance of membrane intervening, as he conducts it onward, is diminished.

3. The hands opposing each other, the surgeon feels confident that the knife glides along the groove; he can, therefore, attend undistractedly to the sensation imparted by the division of the prostate; can better feel when he has completed the division; and when to withdraw his knife.

4. This undistracted attention to the section of the prostate, is aided by the straightness of the staff. The surgeon, operating with the curved instrument, has two movements to execute with his scalpel; one progressive, to get into the bladder; the other curvilinear, to keep in the groove of the staff. With the straight staff he has merely to propel his scalpel; it is the opening of a sinus on a director.

5. Prolapsus of the rectum, if this be an evil, is prevented, apparently by this staff; for, when it is held by the assistant, its extremity rests on the sacrum, pressing the walls of the gut together, and so precluding invagination.

6. As Mr Key remarks, by keeping the blade of the knife parallel with the *ramus ischii*, the pudic artery is not endangered.

7. By augmenting the angle formed by the scalpel and staff, a proportionally larger division of the prostate may be effected.

8. By depressing the handle of the staff the surgeon carries the knife upwards away from the bowel.

Such I deem to be the advantages of this mode of lithotomizing. I have heard some objections. I have heard one of the first lithotomists and surgeons, a gentleman of this city, say, that he should fear lest the contracting bladder might expel the staff, loosely held by the assistant, too far outwards to insure a sufficient section of the prostate upon it, such an accident being less probable when the curved instrument is used. To this I would reply—How far the staff should be carried into the bladder, the finger thrust up the rectum will indicate. The surgeon may, as was practised by myself, fix the end of the staff, beyond the tip of his finger, against the sacrum, requesting his assistant to keep it firm upon that spot. When the operator has entered the urethra with his scalpel, and taken the staff into his own left hand, being secure that it has not yet moved outward, he may assuredly take care that now it shall not. As he depresses the handle, he may readily, with common tact, obviate extrusion. The whole process being now his own, he has an advantage, I think, over the *unsympathising* actions of two persons. Another objection is founded on the absence of the bulge in the perinæum produced by the curved staff. But this I believe to be a favourable circumstance; for this bulge, according to the statements of the best writers, has induced many to cut too high towards the angle of the ossa pubis, and consequently to wound the bulb, to slit up the urethra too anteriorly, and to make too small an opening for the extraction downwards of the calculus.

Mr Key's knife is a little rounded off on the back towards the point, that it may run more smoothly along the groove. With this unpointed end it is not, in my opinion, so easy to enter the groove as with a knife which punctures the urethra; but this inconvenience is more than compensated by the facility with which the scalpel is conducted into the bladder.

In one stage of the operation above noticed, I departed from a rule of Mr Key. He recommends the surgeon, when he is about to introduce a finger into the bladder, to seize the staff with his *right* hand, to pass his *left* fore-finger along the staff, and on this finger to pass the forceps. Conceiving this to require a cramped movement of the arms, I retained hold of the staff with my *left* hand. Yet in some cases this proceeding

would be wrong. Where there is difficulty, requiring force in the introduction of the forceps, it would be imperative to interpose the finger betwixt the bone of the pubes and the instrument. The finger fixed under the pubic arch would thus defend from laceration the connexions of the urethra and prostate with each other, and with the pubic arch. If now the dilatation of the wound should go on to rending, this undesirable effect would occur in the best direction. The surgeon who aims at this security must follow Mr Key's directions, for he could not place his right fore-finger against the pubic arch and pass the forceps along its palmar surface with his left hand.

If, by the process of Mr Key, a section of the prostate is made, sufficient to supersede the use of the blunt gorget, much mischief will be prevented. Some of the most judicious surgeons and cleverest anatomists have concurred to deprecate all gorgets. And although others of equal character have adopted even the cutting gorget, yet I presume that no man ever has been found, who would, from abstract principles, select this instrument. They have deemed it the best where all have been pronounced imperfect.

Even with the blunt gorget frightful mischief has been caused. I have seen it in the hand of a professed anatomist slip off the staff, ascend betwixt the pubic bones and bladder, and occasion death in a few hours. For what purpose is the blunt gorget employed? To dilate the wound of the prostate, or to guide the forceps, or both? Whatever should be the object of the surgeon, I am inclined to believe he may effect it, under ordinary circumstances, by Mr Key's process without a gorget; and that, if he must use one, he will handle it with more precision, security, and effect, if he run it along a straight staff.

Several surgeons present at the operation which I have mentioned, Messrs Scott, Browne, Lee, and others, expressed themselves pleased with the apparent advantages of Mr Key's mode of operating.

Norwich, February 13, 1827.

ART. IV.—*Observations on Bronchitis, chiefly relating to its Pathology and Treatment.* By H. W. BURRELL, M. D. Assistant Surgeon 72d Regiment. (*Continued from last Number.*)

WITH regard to the treatment of Bronchitis, it must vary not only according to the extent and severity of the inflammation, but to the age and constitutional peculiarities of the indi-

vidual. The results of remedial measures in one class of subjects will not justify their adoption in another, though both may have appeared, from the nature of the symptoms, to require similar treatment. Thus, blood-letting may be carried to a considerable extent in some cases, and with perfect safety and advantage, whilst in others the same amount, though apparently more indicated, will be followed by manifest aggravation.

In the treatment of this disease it must ever be kept in mind, that, as we diminish the amount of the cause, we invariably increase that of the effect, namely, the effusion—to which the worst symptoms are owing. In applying our remedies, therefore, more especially depletory ones, the question is not altogether how far the extent of inflammation will allow them to be carried, but how far the probable amount of local and general relaxation that is to follow will justify their adoption; for it is too certain to require any proof here, that the subsidence of inflammation, whether spontaneously, or through the influence of remedies, is the period at which effusion commences, and that this effusion, whether determined by constitutional or by remedial interference, proves, when in moderation, a source of health, and when in excess, a cause of death.

It is fair to conclude, that the quantity and quality of the effused matter will not be altogether regulated by the degree of inflammation, but also by the constitutional or remedial influence which may have operated in reducing the inflammatory tension of the vessels; for there is little doubt that the vessels in a vigorous subject are slower in effecting the secretive stage of the disease than in an old or debilitated one. Hence we may infer that the same amount of inflammation will be followed by a much greater loss of tone in the latter, giving rise to a much more copious effusion, than in persons of greater original or better husbanded strength.

In regulating, therefore, the kind and extent of remedies suitable to this disease, regard must be had to the constitutional and local peculiarities of the patient, arising from age, temperament, habits of life, and previous diseases, more especially those of a pulmonary nature. It is also of the utmost importance to attend to the peculiar features of the disease itself, whether it has been superinduced on a previous subacute or chronic inflammation of the bronchi, parenchyma, or pleuræ, or has been an original and uncomplicated attack. And, above all, it is necessary to consider the amount of local and general inflammatory action, with its duration, symptoms of progress, and peculiar features of decline.

It is chiefly in the decline of bronchitis that the influence of constitutional peculiarities becomes remarkable. During the first and purely inflammatory stage of the disease, the symptoms *cæteris paribus* do not bespeak a more unfavourable termination in one constitution than in another. In both there is cough, dyspnœa, and fever proportioned to the local disease. It is by the period, amount, and danger of the succeeding effusion that we discover its influence. In old or debilitated subjects, the stage of effusion, whether induced spontaneously or by remedies, takes place much sooner, and to a greater extent than in the young and vigorous. Thus it is not unfrequent to see young healthy persons affected with a violent bronchitic cough for several weeks, without any other expectoration than a thin scanty mucus. This, when left to itself, is either succeeded by resolution, and a copious and thicker secretion, or terminates in general pulmonary inflammation.

In proportion as remedies shall have been early employed, more especially blood-letting, the secretive stage of the disease will be sooner established. Whether that is to prove a salutary or dangerous process, will depend on the rapidity with which the effusion takes place, and the remaining local and general strength of the individual. Under great natural or artificial relaxation, whether of the exhalant vessels, or of the general system, the effusion is often so quickly secreted, or, in the absence of expectoration, so rapidly accumulated, as to preclude the possibility of the lungs making effective efforts to unload themselves. This is especially noticed in old persons and very young children, who, with symptoms portending at first no great or immediate danger, are often suddenly beset with all the horrors of suffocation.

The absence of other previous or present pulmonary diseases is one of the most fortunate circumstances that can attend an attack of bronchitis. Wherever bronchitis supervenes on an inflammatory disposition of the lungs, it is always of a much more formidable nature than where, even though in itself more severe, it remains uncomplicated. This arises not from the greater amount of inflammation abstractedly considered, but from the greater afflux of humour, if I may be allowed the expression, to the bronchi, which will sooner or later follow the resolution of this more extensive disease. I need not stop to illustrate this fact; we have but to recur to the rapidity with which plithisis, asthma, and other chronic inflammatory affections prove fatal when they become associated with acute bronchitis, and to the ease and safety with which the latter disease is combated, when it occurs as an original and uncomplicated affection. For the above reasons hooping-cough and eruptive bronchitis

are always to be considered more dangerous when they occur in conjunction with pulmonary inflammation. By attention to these distinctions, the extent and kind of treatment requisite for the several modifications will be determined with more accuracy and safety than if directed solely with reference to the actual amount of disease.

Blood-letting may be employed with advantage in the commencement of nearly all cases; but, for the reasons before adverted to, it is necessary to distinguish well those in which its employment requires great caution from those in which it may be used with nearly the same freedom as in other pulmonary inflammations. As before stated, the most dangerous examples of the disease are generally those in which an acute attack has supervened on a chronic inflammation of the pulmonary organs. Such cases are also the least favourable for excessive depletion. The more the disease is confined to the bronchial mucous surface, the constitution young and vigorous, and exempt from previous diseases, the better, *cæteris paribus*, will it admit of blood-letting.

It is often necessary, however, even under the most favourable aspect of the disease, to husband the general strength; and there is no better proof of this than the unfavourable turn that is frequently given to whooping-cough, especially in very young children, from the excessive use of evacuants. Indeed there is no better illustration of the influence of general and local vigour over the termination of bronchitis than what may be frequently observed under the modern treatment of whooping-cough; and I think it admits of question whether, with a certain class of subjects, the former simple treatment, founded on the supposed spasmodic nature of the disease, was not at least equally successful with that which has been suggested by the doctrine, that it depends on inflammation. But be that as it may, the success of depletory measures in whooping-cough has not been in correspondence with the apparent necessity for their exhibition. An unprejudiced appeal to our experience of this treatment will furnish us with many cases in which, although it has been in strict accordance with the pathology and symptoms, it has nevertheless not been justified by the result.

There is as little question about the inflammatory nature of whooping-cough, as that in numerous cases it readily admits of a spontaneous cure. But the knowledge of the latter fact, together with the unhappy effects which are frequently observed to follow a too free antiphlogistic treatment, seems to suggest a more cautious and discriminate use of evacuants than the strict followers of Dr Watt commonly adopt. The first and inflammatory stage of the disease will, in many instances, admit of

the same spontaneous resolution as the common catarrhal complaints of young subjects; and I have myself, in several instances, witnessed the most untoward symptoms from the undue use of remedies given with the view of accelerating this process, which, especially with very young children, will commonly take place in a manner more compatible with the function of respiration, if left to itself, than if suddenly established by the relaxing operation of remedies. Under an incautious employment of these the bronchial branches are often rapidly filled with effusion, which, on account of the general debility induced by remedies, and obstructed respiration, cannot be expectorated. Hence arises the short, frequent, suffocative cough, and wheezing respiration, which we must ever recognize as an unfortunate substitution for the long, deep, and strong cough, and respiration of the commencement. For although the latter implies a more unsubdued state of inflammation, yet it is safer, under certain restrictions, to protract the duration of this stage, than, by too active measures, to promote a too rapid accession of the stage of effusion, which, as said before, will frequently follow in a more gradual, moderate, and salutary way, if left to the unassisted powers of the constitution.

The above remarks, which more particularly apply to whooping-cough, are not meant to advocate the non-employment of depletory means in the active stage of bronchitis, but to point out, that, as this kind of inflammation often admits of spontaneous resolution, the necessity for carrying such remedies to any great extent is less indicated. And again, that, as the chief danger of their abuse is the influence they hold over the stage of effusion, their extent in all modifications of the disease will be much more safely regulated by reference to this stage, (the nature of which may be in general foretold by the peculiarities of the case,) than if drawn from any direct or collateral reasoning suggested by the actual amount of inflammation.

In young and robust habits, then, labouring under an acute and original attack of bronchitis, blood-letting will be of advantage in the commencement, but must not be repeated with the same freedom as in other thoracic inflammations. In old or debilitated subjects affected with a secondary, or complicated attack, I should, from my own experience, put little faith in general blood-letting at any period, but would rather rely on other antiphlogistic remedies. The inutility and danger of depletion in the more advanced stage of bronchitis, especially as regards the severer varieties of it, will be pointed out in the observations subjoined to the cases which accompany this paper.

There is no remedy so safe, and at the same time so effectual, as the internal exhibition of *Tartar emetic*. If it has succeeded in subduing the violence of pneumonia, we may, with some confidence, make it supersede the necessity of blood-letting in many cases of bronchitis. It may be employed in all stages, and in all varieties of the disease. My much respected friend, inspector of hospitals Dr James Forbes, to whom I feel deeply indebted for much valuable practical information, first suggested tartar emetic to me as being better adapted than blood-letting to lower the inflammatory action of bronchitis; and I am happy to say my experience since has confirmed this opinion. If properly regulated, it controls the force of the circulation for a much longer period than the loss of any quantity of blood which we should feel warranted in abstracting; and it is never liable, like the latter, to the greater objection of permanently reducing the general strength. Besides possessing the power of subduing inordinate vascular action, it determines to the skin and bowels, promotes absorption, and probably acts from its local irritant effects as a revellent.

The exhibition of tartar emetic with very young children requires considerable caution, for a large dose is often retained very long, and produces excessive nausea and depression of the system. To adults I have commonly given it in the dose of one-third of a grain, dissolved in an ounce of water, every hour. This I have of course had occasion to vary; but with most patients the above is the dose and interval I have found to answer best, especially where vomiting is to be avoided. I have found that smaller doses more frequently repeated are more apt to excite vomiting, although in a given time the aggregate quantity has been less; and this appears to be occasioned by the commotion given to the contents of the stomach, as also by the stimulus of repletion. Hence, when we desire an uninterrupted state of nausea, the dose should be large, the interval long, and the patient, as far as regards motion and drink, perfectly undisturbed. The nauseating effects of the remedy are often counteracted, or altogether suspended when it affects the bowels; and this I think is more liable to happen with small doses than with large ones, which seem to obviate this by more readily concentrating their action on the stomach. The affection of the bowels may be removed either by increasing the dose or by opium. When there is any muco-intestinal inflammation present, a very unpleasant diarrhœa is often the consequence of tartar emetic, and under every variation in its exhibition defeats the desired operation of the remedy. It is better in such cases to suspend its use for a time, and allay the irritation of the bowels,

as by persevering we exhaust the general strength, without effecting any material reduction of arterial action.

The remedy next in importance to tartar emetic is perhaps *Hercy's Inhaler*. The benefit likely to be derived from the application of warm vapour to an inflamed surface need not be enlarged on. It is sufficient to mention that this description of inhaler has been found a valuable auxiliary in all acute pulmonary inflammations. In addition to its soothing and relaxing operation on the lungs, its use is commonly followed by a copious perspiration about the head and upper part of the body. It is chiefly in the commencement of bronchitis that it will be found beneficial. In the advanced stages the advantages do not compensate the trouble and dislike which it seems to occasion. Wherever there is great dyspnoea from bronchial obstruction patients almost always refuse to employ it.

As far as my own experience goes, *Blisters* are entitled to little confidence in acute thoracic disease, or indeed in any disease where there is much general excitement. I have seldom seen the irritation, restlessness, and fever, which they occasion, compensated for in any degree by their local advantages. As a proof that they are less indispensable than many deem them, I may mention, that, among the numerous cases of acute pulmonary disease which have come under my care in the last two years, I have not had occasion to use above three or four blisters, and those never before the fullest trial of other means. That I may not be misunderstood on this point, I must remark, that I admit the efficacy of blisters in chronic or subacute diseases; but wherever there is any recent and unsubdued pain and general fever remaining, I think them not only an unnecessary, but also a very injurious addition to the means usually employed. In such cases too they are apt, by obscuring the real amount of the original pain, to tend to dangerous errors; and I have myself been witness to more than one melancholy instance in which the application of a blister to the abdomen in enteritis has led to a fatal security, both on the part of the patient and physician. Blisters, in my opinion, should not be used during the violence of bronchitis, but may be found very advantageous in the latter stages, when blood-letting can no longer with safety be resorted to.

Some benefit may be derived from *Emetics* in the latter stages. I can see little indication for their use where there is a free expectoration; and I think they are commonly too early and freely, and too often without any ostensible object, prescribed in whooping-cough. As it is desirable in far advanced cases to avoid protracted nausea, sulphate of zinc should be employed.

There is only one more remedy which seems to merit attention in this place, viz. *mercury*. From its powers in other inflammations I think it deserving of a full trial in bronchitis, the more especially, as in the aggravated cases of the disease we are a good deal restricted in the use of blood-letting and other evacuant remedies. I have not myself given it so full a trial as I could wish; but as far as that has gone I should be led to expect considerable advantage from an early and complete mercurialism.

In conclusion, medicine may do much in the milder varieties of bronchitis, but in its severer forms there is no acute disease which so often sets at defiance all means of cure, or more forcibly impresses the physician with a conviction of the poverty of his resources. Unless we can suggest some remedy that will diminish or remove the effusion, we cannot hope to avert the fatality of a great number of cases.

I am aware the subjoined cases have been brought under the notice of the Board; but, as they are the severest examples of uncomplicated bronchitis I have met with, and most illustrative of the preceding observations, I feel that little apology is necessary for their repetition here. The first two came under my own care, the others under that of my friend assistant staff-surgeon Dr Birrell, who has kindly permitted me to publish them. I had an opportunity of observing the progress of all of them; and I may say that the many obvious truths, which their unequivocal character afforded, laid the ground-work of all my future inquiries into the disease. I hope to be pardoned for the liberty I shall take of commenting on the treatment of them, convinced that all concerned will feel with me the propriety of exposing error.

Case I.—John Freely, 65th regiment, æt. 40, 15th December 1819, came into hospital late last night labouring under extreme difficulty of breathing, and frequent cough, with pain in different parts of his body, and general febrile excitement. When visited this morning he complained of a load and sense of tightness across the chest; respiration short, hurried, and performed with a wheezing noise, and he appeared as if unable to expand the lungs. The cough was short, occurring in paroxysms, and accompanied by mucopurulent expectoration, somewhat frothy; countenance extremely anxious, with paleness of the lips; general inquietude, and frequent mourning; pulse 100, moderately full, and hard; tongue furred, with urgent thirst; skin hot and dry. Had served fourteen years in India, and had lately returned to this country. Of a short make, well-chested, and rather plethoric; of a sanguineo-phlegmatic temperament. Had been affected with cough and dyspnoea for the last thirteen years. The above severe symptoms attacked him about five days previous to admission, and had since been increasing.

Thirty ounces of blood were drawn, which was much buffed and cupped. Frequently during its abstraction he expressed himself relieved, but repeatedly requested no more should be drawn as he was unable to bear it. He had a draught of ether and hyoscyamus, which afforded some relief; and he was ordered a pill every four hours, half a grain of extract of opium, and five grains of calomel. He entreated that a blister might not be applied, and he was indulged. He evinced great aversion to move, or to be spoken to, and refused his medicine from the attendants. He lay on the right side or back, and seemed to have no inclination for the erect posture. His breathing became hourly more and more laborious, and he latterly actually gasped. He continued in this way till 12 o'clock on the following day, when he expired.

Dissection.—Considerable deposition of fat; turgescence of the vessels of the brain; slight adhesion of the left lung at the posterior part; lungs much congested, and did not collapse on opening the thorax. About one-third down the trachea there commenced on the lining membrane a slight blush of inflammation, which increased downwards, assuming at the bifurcation a highly injected appearance. This became more distinct towards the bronchial ramifications, and in the minuter branches it took on a blood red appearance. The smaller branches were completely blocked up by a muco-purulent matter. With the exception of the above, the lungs were free from disease. The liver was considerably congested, and firmer than usual.

Remarks.—The above was a case of severe acute bronchitis, probably supervening on a chronic inflammatory state of the mucous membrane. The prognosis was from the beginning very unfavourable. In cases of bronchitis, neglected as the above one had been for five days, so great an accumulation of matter takes place in the smaller branches as to preclude the possibility of saving the patient. On the accession of the disease in this case blood-letting to some extent was indicated by the high febrile excitement. But, performed as it was in the advanced stage, it could afford only temporary and mechanical relief to the dyspnoea; and was rather calculated to increase the quantity of obstructing secretion.

Case II.—Henry Perkins, 65th regiment, æt. 43, admitted about 1 o'clock P. M. 24th December 1819; of a sanguine phlegmatic temperament, rather short and plethoric. Has been in India twelve years and returned lately. Has lived pretty temperately, and enjoyed pretty good health till about three years ago, when he was attacked with liver complaint, and was sent to England in consequence. States that five days ago he was attacked with cough; two days after which he became affected with dyspnoea, which has since continued and increased. He at present complains of inability to take a full inspiration, which, when attempted, gives him considerable uneasiness. Cough short and frequent, and accompanied with copi-

ous muco-purulent and frothy expectoration. Respirations 30, loud, and accompanied by a slight wheezing noise; pulse 120, moderately full and strong. Is forced to be in the erect posture. When he attempts to lie down he is immediately threatened with suffocation. He has no pain about the chest, but a great sense of stuffing and fulness. Countenance extremely anxious and pale; lips livid; frequent moaning; tongue white, with much thirst; bowels free; urine thick, and depositing a reddish sediment; skin natural in temperature. Has some pain of right hypochondrium. He never before had cough, or any pain of chest. Ascribes his ailments to cold.

Fiat venæsectio ad uncias triginta duas. R Sulph. Magnesie unciam; Infusi Sennæ, uncias duas; fiat solutio, partitis vicibus sumenda. R Ether. Sulph. semunciam; Muc. Acacie uncias duas; Tinct. Scillæ drachmus duas; Aquæ uncias quatuor; fiat mistura, cujus sumat cochlearia duo unaquaque hora.

7 P. M. Fainted during the blood-letting. Blood drawn buffed and cupped. During its flow he frequently expressed himself relieved, and immediately on its being finished lay in the horizontal posture. At present he can take a much fuller inspiration, and is otherwise very much relieved. Cough less frequent; pulse 100, moderately full and soft; expectoration copious, and muco-purulent. Four alvine evacuations.

R Hydrarg. Submur. grana viginti; Extracti Opii, Tartratis Antimonii, singulorum grana duo; fiat massa, et divide in pilulas quatuor; sumat unam sexta quaque hora.

25th, 11 A. M.—Respiration remained pretty free from the time of last report, but he was extremely restless, and lay in every possible position. At dawn this morning the dyspnœa became much increased. There is considerable labour in respiration, but it is not so quick or loud as on admission, and the wheezing is not so perceptible. Cough frequent; expectoration copious; countenance pale and anxious, and the lips have an exanguious appearance. Complains of a pulsation which is very distressing to him in the epigastrium. It can be felt by the hand, but the pulsations are not sufficiently distinct to be counted. Pulse 110, small, firm, and somewhat irregular. Five stools. Expresses himself better than on admission, but not so well as last evening.

Repetatur venæsectio ad uncias sedecim.

7 P. M.—Fainted during the venesection. Blood drawn much buffed and cupped. Immediately on its being finished, and while in the horizontal posture, he fell into an apparently sound sleep, which lasted for an hour. When he awoke he expressed himself "a new man." Respiration not so loud; cough less frequent; expectoration copious; does not remain above a quarter of an hour in any posture; and is extremely restless, with paleness of face and lips, and much expression of anxiety. Pulse small, weak, and irregular. Does not now feel any beating in the epigastrium; heat of skin lower than natural and the nails have a cadaverous appearance. Complains much of debility. Pills produce nausea.

Continuentur omnia.

11 A. M.—Passed a restless night. Lay in all positions, but chiefly on his right side and back. Respiration not so quick or so loud; cough less frequent; great exhaustion; countenance pale and anxious; lips livid; eyes glossy; heat below natural; pulse scarcely perceptible. When asked how he feels himself, replies, “I am perfectly easy.”

Habeat vini rubri semunciam singulis horis.

27.—Soon after last report he had to be supported in the erect posture, and continued to maintain this position for about two hours, when he gently resumed the horizontal posture, and almost immediately expired.

Dissection.—*Head.*—Slight effusion between the tunica arachnoidea and pia mater. Slight increased vascularity, and about a drachm of serum in the lateral ventricles. *Thorax.*—Free from adhesions, with the exception of slight and partial ones at the posterior part of left lung. Superior surface healthy in appearance, and free from tubercles, distended with air, but not so much so as is usually observed in similar cases. The lining membrane of the trachea, from its commencement to its bifurcation, had a slight blush of inflammation. At this last part it increased, and continued to do so as far as it could be traced, assuming, towards the lower part of the bronchi, a livid appearance, and, in the minuter branches, a still darker hue. The surface of the lining membrane of the trachea and bronchi was covered with a muco-purulent matter, and the smaller branches were completely filled with a similar matter. The lungs were much congested at the posterior part. The liver was congested, and firmer than usual.

Remarks.—The above was also an advanced case of severe acute bronchitis, and probably, as far as regarded the lungs, uncomplicated. The paleness and anxiety of the countenance in this case was indicative of obstructed respiration from effusion,—our object, therefore, ought to have been the removal of this. The first bleeding was perhaps called for, though not to the extent to which it was carried. The second was altogether inadmissible, and probably hurried on the period of dissolution. It will be remarked, that this case and the preceding are corroborative of Dr Hastings' opinion of the frequent connexion of bronchitis with hepatic disease.

Case III.—Serjeant Anthony Matthews, fourth dragoon guards, æt. 41, 24th December 1818, labours under great oppression of the chest, and severe dyspnœa. Respirations 28. Says he is unable to make a full inspiration from the sense of oppression in his chest. Lies on the left side, but says he can do so on either; but is unable to lie on his back from a sense of suffocation. Says the breathing is less difficult in the horizontal than in the erect posture. The seat of oppression is from the fifth rib of left side to the left hypochondriac region, and to the ensiform cartilage, at which last

situation there is a very strong pulsation, beating 104 times in the minute. The pulsation of the radial artery at both wrists is 84, and regular, but small and weak. Has a very troublesome teasing cough, accompanied with scanty expectoration of muco-purulent matter; has no pain of any part; feels cold; skin natural as to heat; bowels free, by salts taken yesterday; tongue pretty clean; no thirst; urine natural; temperament, sanguineo-phlegmatic. Has served twenty-three years in England, Spain, and Portugal; always enjoyed good health; only once in hospital for the venereal disease, about twenty years ago. About a fortnight ago contracted a gonorrhœa, for the cure of which he employed copivi balsam, and spirit of nitrous ether. A few days afterwards his present ailment appeared; but he was able for his duty till three days ago, when the symptoms became as above described. Did not perceive the pulsation in the epigastrium till last evening. Has used no medicine, except two or three doses of salts. Was cupped on both sides of the chest this morning, when admitted, and about ten ounces of blood were taken from the arm. The blood much buffed and cupped.

R. Aceti Scillæ uncias tres; Tinct. Digitalis guttas quadraginta; Tart. Antimon. granum; Syrupi et Muc. Acaciæ, singulorum unciam; aquæ uncias tres; fiat misturâ, cujus sumat unciam tusse urgenti.

Admoveatur Emplastrum Lyttæ pectori, et hora somni sumat haustum e tinct. camph. comp. drachma semis.

25th.—Slept at times during the night. Lay on either side, but never on his back. Cough recurred at pretty long intervals, but was then very teasing, and never ceased till a quantity of purulent-like matter was discharged. At present, when asked how he is, says he is very easy, and inclined to sleep. Respirations 38, and short, and he seems unable to make a full inspiration, which at times is attended with a wheezing noise. His forehead is covered with a cold sweat, but the rest of the surface is of a natural temperature. The pulsation at the ensiform cartilage is quicker than yesterday; but, from his continually coughing, its frequency cannot be ascertained. At both wrists the pulse is now beating 120, regular, but very feeble.

He died about three hours after this report was taken, viz. at 2 P. M.

Dissection.—Head.—Considerable congestion of the vessels. Lateral ventricles distended with serous fluid to the amount of an ounce. Choroid plexus paler than usual. The blood-vessels ramifying on the spinal marrow much congested, and there was effusion between that substance and its sheath. Thorax.—Lungs externally of a healthy appearance, and free from adhesions; but there was considerable congestion of blood in both, and they did not collapse, their cells being distended with air. The epiglottis was slightly vascular; the lining membrane of the trachea highly so, and possessing the appearance of a red villous surface. This was comparatively slight at the upper part, but increased towards its bifurcation, and in some places it had a dark livid appearance, especially in the minuter branches, which contained a considerable quantity of frothy

mucus, mixed with purulent-looking matter. Heart and pericardium free from disease. *Abdomen*.—Congestion of the liver and right kidney, with strong adhesions between those two viscera. Several partial thickenings of the peritoneal covering of the liver.

Remarks.—The above was a pure and uncomplicated case of severe acute bronchitis in a very advanced stage. The system was evidently in a state of extreme exhaustion and collapse on the man's admission, and any depletory measures were therefore injudicious.

Case IV.—Dominique Trisney, aged 25, admitted 24th December 1818. His countenance is expressive of great anxiety; his lips are alternately pale and livid; and respiration is extremely difficult and frequent, viz. 32; inspiration attended with a wheezing noise; he can lie equally well on either side or on the back; has no pain, but great sense of constriction and fulness in the chest, and he describes his uneasiness as extending along the whole internal surface of the sternum; his cough is severe, but not frequent, and is always terminated by expectoration of a thick yellow matter; pulse 110 and sharp; tongue white; bowels regular; heat of skin increased; temperament phlegmatic; habit of body rather weak. Birth-place Venice. Has served eight years chiefly in the East Indies, Surinam, and Holland; served also in the French army seven years under Napoleon. In all the above period he enjoyed good health. The present affection came on seven days ago with cough and scanty expectoration, but he was able for his duty until yesterday.

Fiat venæsectio ad uncias triginta duas. Sumat statim sulphatis magnesiae unciam.

25th.—He continues in the same state as on admission; pulse 100, small, but softer; three stools; skin dry and hot; blood shows no inflammatory appearance.

Applicetur emplastrum cantharidis pectori; Applicentur hirudines viginti inferiori cervici. Habeat hydrargyri submuriatis grana sex et pulveris antimonialis grana tria in pilulæ forma.

Evening.—Respiration continues quick and loud, but the sense of constriction is somewhat relieved; pulse 100; several alvine evacuations. Leeches bled freely.

Sumat pilulam e calomelanos granis tribus tertia quaque hora, et misturæ salinæ semunciam alternis horis.

26th.—Slept better; respiration as yesterday; several stools; pulse 102, and small; perspired freely during the night, but at present the skin is hot and dry. Has taken seven pills.

Continuentur medicamenta, pilulis exceptis, et adhibeatur frictio per spinam ope unguenti ex unguenti hydrargyri uncia, camphoræ et pulveris opii, singulorum semidrachma.

7 P. M.—The symptoms much aggravated during the day; pulse 100, and sharp; skin hot and dry; thirst urgent.

Repetatur venæsectio ad uncias triginta duas. Continuentur alia.

27th.—Slept little; no alteration in his breathing; cough not frequent, but still accompanied with copious muco-purulent expectoration.

toration ; pulse 88, small, and rather feeble ; tongue very white, with thirst ; several alvine evacuations during the night ; mouth not sore. The first sixteen ounces of blood taken last night presented a buffy appearance.

Adhibeatur frictio cum unguenti hydrargyri drachma bis indies.

R Pulveris Rhei grana quinque, cretæ preparatæ, pulveris zingiberis, singulorum granu quatuor ; fiat pulvis, ter indies sumendus. Habeat quater indies haustum ex aceti scillæ semidrachma, tartratis antimonii quarta parte grani, mucilaginis et aquæ singulorum semuncia.

Evening.—Respirations 38, and louder ; pulse 115, and very firm ; skin hot and dry, except the forehead, which is covered with a cold sweat ; cough more troublesome, and expectoration very copious ; lies in the horizontal posture, and on either side ; bowels very loose.

Emittantur e brachio sanguinis uncia sedecim ; applicetur emplastrum cantharidis superiori pectori.

28th.—Twenty ounces of blood were drawn, but his symptoms were not at all relieved by it. Blood cupped and buffed. To-day the symptoms are rather aggravated. Respirations 38 ; pulse 110, small, and rather sharp ; several yellowish stools. Half an ounce of mercurial ointment was rubbed in yesterday, and his mouth is now very sore, but there is no salivation.

Repetatur frictio hydrargyrica.

Evening.—All the symptoms are greatly increased ; respirations 44, and louder, and he is now obliged to be supported in bed. Pulse 120, small, and weak ; cold sweat over the whole surface ; cough more severe, and frequent ; mercury rubbed as directed, but there is no salivation.

29th.—Died this morning at four o'clock.

Dissection.—*Head.*—Membranes and substance of the brain of a healthy appearance ; choroid plexus extremely pale. Along the whole surface of the medulla spinalis the blood-vessels were much distended, and a considerable quantity of fluid was found between that substance and its sheath. *Thorax.*—On depressing the head and neck muco-purulent matter escaped by the nose. On dissection a considerable quantity of similar matter was found in the fauces, and a still greater quantity in the upper part of the larynx, along the whole trachea, and in the ramifications of the bronchi as far as could be traced. The quantity was greatest in the minute ramifications of the bronchi, and was evidently sufficient to obstruct respiration. The trachea was quite free from inflammation ; and in some places only of its minuter ramifications there was an appearance of vascularity, but not amounting to inflammation. The lungs externally appeared quite healthy and free from adhesions, except the upper lobe of the right lung, and these were evidently not recent. On cutting into the upper lobes of both lungs a very considerable quantity of frothy mucous escaped, which was not the case with the lower portions of the lungs. Heart and pericardium free from disease. *Abdomen.*—Gall-bladder empty ; spleen at least

double its usual size ; omentum destitute of fat ; and the appendiculae epiploicae very small and indistinct.

Remarks.—The foregoing case is also an uncomplicated one of severe acute bronchitis in a very advanced stage, as was denoted by the livid lips—constriction of chest—laborious respiration, with wheezing and frequent cough, terminated by expectoration of muco-purulent matter—all descriptive of excessive accumulation of matter in the bronchial tubes. It presents many interesting points for our consideration. 1st, The insidious nature of the disease, with the absence of all alarming symptoms till the fifth day after its attack, during which, it may be fairly presumed, there was present a much greater degree of bronchial inflammation than was found on dissection. 2d, The great accumulation and danger of effusion, notwithstanding that its cause, the inflammation, had in a great measure, if not altogether, subsided. 3d, The very satisfactory evidence, that, whilst by blood-letting and other evacuations we were reducing the inflammation, we were increasing the danger arising from the effusion. 4th, The very probable influence of a weakly constitution in rendering the result fatal, which evidently did not arise from the amount of inflammation, but from unexpectated secretion.

To conclude, the evident indication in advanced cases of bronchitis is the removal by expectoration of the matter accumulated in the bronchial tubes, which is the obvious cause of all the outward symptoms,—and not so much the removal of the inflammation, which is of minor importance, and should attract no more regard, than as a remote cause of the danger impending. Contrary to general rule, our attention is to be directed to the removal of effects rather than causes ; for it is evident that any attempt to subdue by depletion the inflammation at this period, must, by reducing the general strength, not only weaken the chance of the bronchi being freed from obstruction by strong efforts at expectoration, but must defeat for a time, and that time of the most vital importance to the patient, the very object we aimed at, viz. the diminution of the secreted matter. For in this, as in other thoracic inflammations, the removal by blood-letting of the inflammatory tension of the blood-vessels disposes the exhalant arteries of the mucous membrane to increased secretion, and thus augments the very evil, to the speedy expulsion of which our chief endeavours should be directed. These considerations, coupled with the bad success which I have observed to attend the practice, lead me to pronounce blood-letting in the advanced stage as a remedy fraught with dangerous, if not fatal consequences. The only

period of the disease at which it is admissible, and even then it requires to be administered with caution, is at the commencement, when there is a comparative absence of effusion; for the effusion in a very short time presents itself, puts a limit to the use of farther evacuants, and imperiously calls for the adoption of other means. It is difficult to determine the class of remedies best suited to such advanced cases; and under the present bad success which attends our practice, physicians may be justified in trying even doubtful ones, keeping in mind, however, that of all remedies of this kind blood-letting appears to be the least applicable. To free the bronchial ramifications from an adventitious matter presenting a more or less complete obstruction to the passage of air to and from the lungs should be our curative indication.

Lifford-Donegal, 23d December 1826.

Errata in the former part of this paper.

Title, for *H. W. Burrell* read *W. H. Burrell*.

P. 296, l. 5, for *cutaneous* read *coëtanous*.

ART. V.—*Observations on the Unmiæd Effects of Mercury on the System, with a few Practical Remarks on some of the most important Tropical Diseases.* By ANTHONY MUSGRAVE, M. D. Physician to his Excellency the Governor, and Surgeon-General on the Colonial Staff of Antigua, formerly President of the Royal Medical Society of Edinburgh.

To the Editors of the Medical and Surgical Journal.

GENTLEMEN,

I HAVE been a good deal surprised, since my arrival in Edinburgh, at observing, in the course of conversation with my professional friends, how very generally the opinion obtains, that the more aggravated affections of the bones, periosteum, throat, and skin, which we were formerly accustomed to regard as *venereal*, are in reality to be ascribed to the pernicious influence of *mercury* on the system,—an opinion which is entirely at variance with the results of my own observation, during nearly twelve years' residence in a climate where this remedy is extensively employed. I must, therefore, with your permission, avail myself of the ready channel afforded by your Journal, and lay before the profession a summary of my experience, as it bears upon the point in question.

Permit me, however, to explain, *in limine*, that it is by no

means the object of this communication to attack the *non-mercurial treatment of syphilis*. On the contrary, I at once admit that Dr Thomson's publication, and the writings of Mr Rose, Dr Hennen, Mr Guthrie, and others, who have been more or less its advocates, were long ago so far successful in persuading me of the sufficiency, if not of the *expediency*, of this practice, that I was induced to refer it to the test of experiment in the Colonial Hospital at Antigua. This happened so far back as the year 1819; and having completely satisfied myself, in several instances occurring among seamen, that chancre, accompanied by suppurating bubo, would heal up kindly enough under the antiphlogistic regimen, aided by rest and posture, I put the patients through mild courses of the quicksilver pill, and then dismissed them;—but not till then, as I did not think that sufficient evidence had been up to that time accumulated to warrant me in exposing these men to what might prove to be the secondary consequences of such treatment. For the same reason I did not venture in private practice to dispense with a mode of cure, which early instruction had led me to deem essential, and daily experience gave me no cause to regard as either ineffectual or unsafe, when conducted with circumspection,—without which the practitioner must always be dangerous, exactly in proportion to the power of the medicine he prescribes. The value of the lancet and opium as therapeutical agents, few, I presume, will be disposed to question; but is it not equally undeniable that fatal exhaustion or stupor may be the consequence respectively of the excessive or even mistimed exhibition of either?

That mercury, however, will, in certain states of a constitution long charged with the venereal virus, aggravate those very symptoms which on other occasions it is found to relieve, is a fact which no man of ordinary ability can doubt after having been engaged in practice for a very limited period; and I have myself, when consulted in inveterate cases, repeatedly derived both gratification and professional credit from advising an instant omission of all mercurial medicines, and the substitution of sarsaparilla or the nitric acid. Of phagedenic chancre again, I recollect at this moment more than one case in which the process, rapidly destructive under the use of mercury, was at once and permanently arrested by discontinuing this remedy, and interposing free venesection with the strictest antiphlogistic treatment, both constitutional and local. Whatever, therefore, may be my modified and impartial opinion, it is abundantly manifest that I entertain nothing like *prejudice* on either side of this question; and had the prevailing doctrines been *distinctly* confined to the peculiar operation of the sup-

posed specific when employed against a syphilitic taint,—had it been even inculcated that the amalgamation, as it were, of the two poisons produces a new virus *sui generis*, differing in its action from both constituents, and giving rise to more lamentable effects than either,* I am persuaded—so ably and zealously have I heard them expounded—that I should scarce have escaped from your city other than a decided convert to these views. But, if I am not mistaken in my estimate, a majority of the profession in Edinburgh go much farther than this.—They believe, or at all events suspect, that *Mercury* is the SOLE parent of that loathsome and malignant progeny which has been hitherto, as they conceive, very injuriously confounded with the purer *Veneral* race, merely on account of some distant resemblance—and it is against the farther dissemination of *this* less harmless suspicion that I feel it to be imperative upon me to protest. It emanates however from sources in the highest degree influential.—It has already wrought incalculable mischief by instilling doubt and alarm into the public mind as to the properties of an invaluable remedy,—and it can therefore be successfully counteracted only by a direct appeal to FACTS.

Assuming for a moment this question to be one on which you were entirely without bias, and it became an object to you to ascertain beyond dispute any given effect of mercury, whether beneficial or injurious; what part of the world, let me ask, provided all were equally accessible, would you select as the field of observation and inquiry? Would you content yourselves with an investigation instituted by a class of practitioners here, who sparingly, and with hesitation, prescribe its various preparations by the grain and the half grain?—Or, would you not deem it more satisfactory to ascertain the consequences of its employment under a tropical sun, where, setting aside much deplorable *abuse*, even the most cautious among us,—keenly alive as we are to the inconvenience, nay, the absolute danger of its undue accumulation in the system,—are yet frequently compelled by paramount necessity, arising from past experience of the otherwise fatal result, to administer calomel *by the scruple*, or to repeat smaller doses of from five to ten grains at intervals so brief, as to be justified only by the consideration that we decide between two evils, the greater of which is *death*?

Or,—to put the query in another shape,—Let us suppose that you had strong grounds for conjecturing that some peculiar morbid affection,—it little matters what,—was the offspring of a

* This would be somewhat according to Sintelaer.—See an excellent Analysis of the Scourge of Venus and Mercury, London, 1709, and other works on Syphilis, Pseudo-syphilis, and Mercury, in the Med. Chir. Review for December 1821.

certain agency, which had hitherto eluded suspicion; the effect in question being universally attributed to causes of a different kind. Would you, in such a case, look for a confirmation of your conjecture, in the more general prevalence of this affection, towards districts where the influence of the suspected cause was only *partial*?—or would you not rather resort to those where you knew it to be in *full* and *extensive* operation?

The replies which can alone be offered to queries such as these follow too palpably to admit of hesitation, even on the part of my opponents. In order, therefore, to establish a conclusive case against them, framed upon admissions thus extorted from themselves, it will be merely necessary for me to show, *first*, that a practitioner, *ceteris paribus*, would see as much mercury exhibited within *one* year in the West Indies as another could possibly do in *five*, whose sphere of observation was limited to Edinburgh and its neighbourhood: and, *secondly*, that, notwithstanding this lavish consumption within the tropics, we, who have laboured there for a series of years, have not only failed to discover those pernicious consequences for which the anti-mercurialists contend, but have had constant occasion to remark, that examples of what is called secondary syphilis are comparatively rare, under circumstances which, had they the shadow of foundation for their suspicions, ought to render such cases as numerous, or nearly so, as the sum of inhabitants in an island.

I pledge myself to do this—and to show further, that the unmingled venereal poison is fully capable, through neglect or concealment, of effectuating the very consummation of that mischief which they hold to be the “martyrdom of mercury.”

With a view then to establish, in the least exceptionable manner, the superiority of the West Indies as a field for observing the effects of mercury, allow me to take a very hasty glance at the practice which at this moment prevails in regard to our principal diseases.—I shall notice them in the order of their general importance.*

The mercurial treatment of the *Fevers* of warm climates has little novelty to recommend it. But when I mention that the manual of Chisholm, and the works of Johnson and Armstrong, are held in general estimation as the best guides to which those in practice can refer, it were superfluous to add, that the number who confide in it is not likely to be diminished;—

* The observations throughout this letter must be taken as specially referrible to Antigua, as my practice has been altogether confined to that Island; and the diseases will follow according to the order in which, from the *frequency* of their occurrence, they have claimed my attention.

and, undoubtedly, in a majority of instances, its well-regulated application will amply repay this confidence. But here, as elsewhere, the nicest discrimination must be exercised, if we would steer wide of the shoals and quicksands of empirical routine.

In the more concentrated form or higher grade of fever, as it affects Europeans, which has acquired the common denomination of *Yellow fever*—I mean the *continued*, and, if I may be allowed the expression, the more *open* type, (such as prevailed in Antigua in 1816, and of which a history, sketched for the information of my respected friend Dr Ferguson, has been long before the public,*)—my experience leads me to believe, that mercury, administered with a view to the saturation of the system, may be judiciously dispensed with; and an opinion to this effect, which I have since seen no reason to alter, will be found in the article alluded to. In cases like these we find the heat to be intense and general—the arteries, as far as the finger can trace them, have their calibre fully distended, showing the blood to be equally distributed among their extreme ramifications.—In a word, we have to rescue our patient from urgent inflammatory action, not to obviate the more insidious, but not less destructive consequences of congestion.

The immediate and rapid abstraction of blood from the arm, or temporal artery, or both, to an extent to be limited, less by the quantity, however large, than by its decided effect upon the circulation, as deduced from that degree of faintness which is accompanied by copious and general perspiration;† active purgative combinations, into which calomel must liberally enter; and the external application of cold water, under the form of affusion or sponging, with occasional aid from the razor and blisters, are here to be mainly depended upon, and will, in nine cases out of ten, prove sufficient to save our patient, when employed within twenty-four hours from the attack. But we are liable more frequently to a form of fever, peculiarly calculated, from its masked approach, to lull the patient into mistaken security, and thus to baffle, in its subsequent progress, the best directed efforts of our skill; and this form is undoubtedly treated with greatest success by the rapid induction of ptyalism.

Sporadic attacks of a more or less deceitful type are indeed annually to be met with in some of the low swampy situations

* See account of the yellow fever as it appeared in Antigua in the years 1816–17, by A. Musgrave, M. D.—*Med. Chir. Transactions*, Vol. ix.

† When the patient becomes faint after a first and early bleeding, before perspiration breaks out, the finger ought only to be placed over the orifice till the horizontal position, and a little cold water, cause the stream to flow freely again. During the epidemic I am about to describe, I more than once drew three pints of blood in this way from plethoric habits with the happiest effect, when consulted immediately on the accession of the first paroxysm of fever.

with which Antigua unfortunately abounds ; but, towards the close of 1823, an epidemic of this kind appeared to an extent which spread the utmost consternation among all classes of inhabitants ; for, singular to say, it evinced no predilection as usual for our European visitors. The whites, the coloured, and the blacks, males and females, infancy and old age, were alike susceptible of the malignant influence of the air, although that influence might be exerted with varied degrees of intensity.

The disease was commonly ushered in by a mild intermittent paroxysm of brief duration. The chilly fit, occurring generally at bed time, was succeeded by moderate heat, and this again terminated by sweating towards morning ; so that, among the planters, for example, who were the principal sufferers, the patient would rise at day-light, take his breakfast as usual, and proceed to his accustomed occupation, without attaching the slightest importance to the indisposition of the night. The same would be repeated on the following evening with a similar solution of a rather aggravated paroxysm. But the third return, although commencing as before, would speedily be followed by some marked derangement of the sensorium, which was probably the first signal for demanding medical advice. The individual was then commonly found talking wildly, with the heat concentrated about the head and trunk ; the extremities cool, and covered with a greasy dampness ; the pulse at the wrists extremely quick and small ; respiration hurried ; tongue brown and dry, or covered with a frothy slime ; but the stomach generally retentive. Black vomit, as far as I know, was *never* observed ; and discoloration of the surface with hemorrhage in *one* case only.*

These symptoms too often advanced with rapid strides to their termination in death, preceded by low delirium, or coma ; sometimes within forty-eight hours, generally before the close of the third day from the last accession, which completed five from the first moment of complaint. Life was rarely protracted beyond this period without ultimate recovery, although convalescence might be occasionally tedious.

Such, in broad outline, were the prominent features of this singular modification of endemic disease—we must call it a *malignant intermittent*—in which so little resemblance can be traced to the yellow fever of authors. I have no difficulty, however, in regarding it but as an opposite degree of the same continuous scale, modified by agency which is beyond our ken,—for, although I have roughly delineated the phenomena exhibited by

* Among our younger patients, convulsions very commonly ushered in the alarming paroxysm.

a very great majority of cases, here and there an example did occur in European habits of a form either continued *ab initio*, or preceded by one slight paroxysm only, and yielding to the vigorous treatment I have already described; and, among the slaves, we had an opportunity of observing every variety of the simple intermittent, which mingled itself by almost imperceptible gradations with the reigning epidemic.

I ought long ago to have recorded a full detail of the origin and progress of this visitation, which extended from October 1823 to January 1824; but what I have now said will suffice for my present purpose, which is merely to convey some slight idea of the kind of fever which I hold to be the proper subject of mercurial treatment. On this occasion indeed I am persuaded, that where no assistance was obtained, as was too frequently the case, till after the third and dangerous paroxysm was established, any improvement which subsequently took place was chiefly to be ascribed to the operation of this mineral; nor am I aware of a single recovery, under these alarming circumstances, where recourse had not been had to its use. By its unrivalled power in equalizing the distribution of blood, the accumulated mass, lying stagnant in the almost palsied organs, was gradually put in motion. These organs, consequently, resumed their suspended functions; the wonted secretions were again established, and, being copiously poured forth, the system thus became relieved from an overwhelming oppression. Such at least appeared to me to be the *modus operandi* in favourable cases. Salivation I regarded as an indication of the full action of the medicine, and therefore hailed it as the harbinger of good. I did not, however, believe it to be necessary to success,—nay, I could have wished it excluded from its effects; and, under this impression, I never failed to expunge the mercurial from my prescriptions as soon as the rising of the pulse and the incipient restoration of heat to the extremities gave me reason to conclude that the object at which I aimed was about to be accomplished.

Calomel was the preparation generally resorted to by all classes of the profession, upon the principles, under the restrictions, and in the doses laid down by Chisholm and Johnson; and, before ptyalism was set up, which is so much insisted upon by these authors, seldom less than one hundred, often *hundreds*, of grains were accumulated in the system.* In my own prac-

* “In conjunction with cold thus remedially directed, calomel, in every severe case, and in every stage of the disease except the last, was largely exhibited. In some of those whose terminations were fortunate, from 300 to 500 grains were exhibited; and in no case did its conjunction with the cold treatment appear either during the illness or convalescence of the patient to have been injurious. From five to twenty grains,

tice, I found that a liberal addition of camphor economised the quantity of mercury, not only by accelerating its effect through a stimulus at once general to the system, and local to the mouths of the absorbents along the digestive tube, but also by exerting over the purgative and griping effect of calomel a controlling power second only to that of opium, if indeed it be not altogether equal. My common prescription, therefore, after unloading the bowels, was two scruples or half a drachm of calomel, with half a drachm or a scruple of camphor, divided into twelve pills, and of these two were directed to be taken every second, third, or fourth hour, till some marked improvement was observed. No practitioner, however, who deserves the name, would blindly pursue one determined system in the teeth of conflicting indications; and it is therefore scarcely necessary to state, that this plan was in each instance modified or assisted internally by such of the simpler articles of the *materia medica* as circumstances seemed to render advisable; and that externally the warm bath and frictions, blisters and sinapisms, were assiduously called in aid for the purpose of soliciting the blood from the viscera to the surface. But I must not omit some special notice of the lancet and bark, lest I become amenable to a charge either of ignorance or neglect.

After many successive deaths had rendered the alarm general, and application was made for assistance at the first onset of the disease, a single bleeding, to its fullest effect upon the circulation before the orifice was closed, was uniformly successful either in at once putting the symptoms to flight, or in so modifying their subsequent character, that the identity of the attack with the prevailing epidemic was hardly to be recognized.* At a later period, too, advantage appeared sometimes to follow venesection, or, more frequently the abstraction of a few ounces of blood from the temporal artery in urgent affections of the head; but, taking the sum of my experience on this and other occasions, I could wish to make it the means of impressing upon my junior brethren in the West Indies, that much danger is incurred by using the

according to the age of the patient, or the severity of the attack, were given in pill or powder, combined with *pulvis aromaticus*, or the aromatic electuary, every two, three, or four hours, until the bowels were freely evacuated, and the system affected, or until perseverance in the remedy was evidently unavailing. This practice was eminently successful, although cases did occur where even salivation, though early induced, could not avert the fatal termination of the malady."

See an admirable history of the malignant epidemic which raged in the Island of Montserrat in the year 1821, by Richard H. Dyett, M. D. published among "Facts and observations in refutation of Sir Gilbert Blane's doctrines as to the contagious nature of Yellow Fever, by A. Musgrave, M. D. of Antigua."—*Med. Chir. Review* for March 1824.

* This alteration of type under the operation of remedies affords one among the many proofs, that all these varieties of fever are only links of the same chain.

lancet too hastily after fevers of any kind have at all advanced in their course. It is further of essential importance to remark, that, when the type under review is the subject of treatment, and the propriety of bleeding is considered with reference to the duration of the disease, this duration must be reckoned, not from the last chilly fit which ushered in the persisting symptoms, but from the first intermittent paroxysm, however slight it may have seemed to the patient; for by the latter, which is the *correct computation*, the third, fourth, or fifth day may have arrived before a careless observer would suspect that any thing like serious indisposition had commenced. When matters have unfortunately gone thus far before we are made aware of the real danger of the attack, it is now my steady conviction, that it will be invariably more judicious to confide the safety of our patient to calomel and camphor, with due regard to purgatives and external adjuvants, than to risk an attempt to move the supervening congestion, however menacing, by bleeding. The indications at this period are beyond measure fallacious; and, if a vein be imprudently opened, it will too probably happen that the distress, whether general or local, which prompted the measure, may indeed appear to subside, and the practitioner be induced for a time to congratulate himself on its success; but, before or soon after the arm is tied up, a copious sweat will break out, which, becoming more and more profuse, with a corresponding sinking of the pulse, the patient will expire within twenty-four hours, in perfect possession of his mental faculties, and complaining of no sort of uneasiness beyond that which is inseparable from dissolution. I could wish it to be also remembered, that the same unexpected sinking may be the consequence of venesection, even at a much earlier period of *intermittent* fevers, provided a spontaneous solution of the paroxysm be on the eve of taking place; for I have seen more than one remarkable instance where the constitutional effort to throw off the heat by perspiration, co-operating unluckily with the powerful relaxation which follows the loss of blood, proved sufficient to reduce the patient to an alarming and even irretrievable state of exhaustion.

It is doubtless difficult to understand—to me it appears nearly inexplicable,*—why a cause, which dissection afterwards shows to have operated by a process of local disorganization, should, in the first instance, manifest its action under the mildest and most harmless form of fever, with periods—as far as the eye,

* M. Dufan, in his paper on “*Intermittent Irritations*,” has communicated to the profession some interesting facts, but he has by no means *explained* them. (*Journal General de Medicine. Med. Chir. Review*, April 1827.)

the touch, and questions can detect—of *complete* intermission; and yet that the progress of the structural mischief—if we may judge from the aggravation of each successive paroxysm, and the sometimes inconceivably rapid termination of the last in death,—should continue, without interruption, during these apparent cessations even of functional disturbance. Perhaps, too, it is not the least surprising fact connected with this point, that bark, a remedy to which we are in general so little disposed to commit visceral inflammation, should, under such circumstances, manifest its accustomed power; for whenever we had an opportunity, during the epidemic under consideration, of putting this power fairly to the test, we certainly had no reason to be dissatisfied with the result. Three cases I particularly remember, in which the paroxysms, (not congestive, however,) continued to recur after ptyalism was distinctly brought on, and yielded at last, or appeared to yield, to a drachm of the powder given in a wine glassful of the decoction, as often as the stomach could bear the dose, during the absence of fever. Unfortunately we had not then procured the sulphate of quinine, of the superiority of which, over every other preparation, I have since obtained highly satisfactory proofs, by extensive trial of its effects.

These last cases, I may remark, are in utter discordance with that *dogma* which assumes the absolute incompatibility of the febrile with the mercurial action; but I do not apprehend them to be equally irreconcilable with the idea I have myself adopted of the salutary operation of the latter. At any rate, it is sufficient to know that similar occurrences are incalculably rare; nor can I see why the maxim of law, that “*Exceptio probat regulam*,” should not also be extended to medicine.

But I find that this subject has already led me too far from the professed object of this letter, and I shall therefore only add, that during about ten weeks that this epidemic prevailed, I had occasion, among my own patients, to treat *fifty-nine* whites and *fourteen* coloured persons. Of the former *six* died, and of the latter *two*. In consultation I saw *thirty-five* cases, of which *fourteen* proved fatal, making an aggregate of 109 cases, with twenty-two deaths, or a proportion of about *one* in *five*.

Our cases among the *slaves* must have more than doubled this number; but as they fell chiefly under the care of Dr Sheriff, now of Dominica, except when alarming symptoms demanded my personal superintendence, I have preserved no accurate memoranda of that class. Some fatal terminations

occurred here also, but by no means in equal proportion to those I have referred to, among the white and coloured inhabitants.*

Next in importance to fever, in Antigua at least, is the *St Anthony's Fire*, as it is there generally denominated; the *Rose*; *Erysipelas*; the *Elephantiasis* of Hillary; the *Glandular Disease* of Hendy, or, as it would be more correctly termed, the *Migratory Inflammation of the Lymphatic System*. This alteration of the name would have the advantage, always desirable, of comprehending the pathognomonic feature of this singular affection—I mean its tendency to metastasis—which has not hitherto been sufficiently noticed. Indeed, the *St Anthony's Fire* has always appeared to me to be as completely a constitutional disease, as dependent upon a peculiar diathesis, and as prone to migration, as gout itself. Like gout, too, I believe it to be the offspring of causes in some instances hereditary, in others derived from the habits or circumstances of the individual.

An inexperienced practitioner, who sees this complaint in its acute and early stage, would have some difficulty in conceiving it to be the first link in that chain of morbid alterations of structure, which terminates at length in the unseemly Barbadoes leg—the *bucnemia tropica* of Mason Good. Its first invasion—and the accession may be repeated under a similar form for a considerable period—is announced by very severe rigors, speedily followed by febrile heat, so pungent, that I have often been able, from these circumstances alone, to predict the supervention of the distinctive inflammation, even when the individual had never before been subject to its attacks. This inflammation, sooner or later, within forty-eight hours, (if not primarily affecting the viscera,) betrays itself in the scrotum, the mamma, or some part of the extremities, most frequently about the ankle, or high up the thigh among the inferior glands of the groin.

Although commonly circumscribed at first, it not unfrequently diffuses itself over the whole limb, or even extends to the contiguous surface of the trunk; and when the glands themselves are not involved, indurated cords, extremely painful to the touch, can be traced to the nearest cluster. But whatever may be its original seat, the patient is never secure, while the constitutional disturbance subsists, from a sudden retrocession to some vital organ. I have seen it, in the same case, translated from the

* It may be as well to suggest that, by noting the greater mortality among the consultation cases, I cannot have the slightest intention to insinuate that my own treatment was more judicious than that of my brethren, as these cases were divided among many practitioners, and must obviously have been an aggregate of the most unpromising on the list of each.

scrotum to the head ; from thence, after a few hours, descend with the rapidity of lightning to the abdomen,—again migrate to the chest, to return perhaps to the encephalon, and prove fatal there ;—or, under more favourable circumstances, resume its comparatively harmless situation, and run its subsequent course as if nothing untoward had occurred ; and, while occupying the different cavities, giving rise to the ordinary symptoms of acute inflammation of that particular viscus which it had selected for its temporary abode.

Now, long before my time in the West Indies, the best informed practitioners had ascertained, by experience, that, in addition to warmth locally applied, and the ordinary purgative and febrifuge medicines, mercurial saturation of the system was the best means of securing the patient against this erratic disposition ; and, accordingly, ptyalism was the usual companion of convalescence from an attack of this very common complaint.

As all young men ought to do, who have not unexceptionable motives for an opposite line of conduct, I adopted the practice of my seniors, and have never had cause to question the correctness of their conclusions ; for I remember no case of fatal metastasis after swelling of the gums had been unequivocally observed ; a subsidence of the constitutional symptoms having invariably either preceded, or been simultaneous with this effect, even when the local inflammation has continued a course which terminated eventually in suppuration. I am, indeed, strongly disposed to believe, that, since the mercurial treatment has become general, the number of cases ending in a local and permanent deposition of lymph has been materially lessened ; as I could name many individuals subject to this disease, affecting the leg, the thigh, or the scrotum, whom I have attended for a number of years, and who yet continue to shake off each successive return, without retaining the slightest trace of its having existed ; or, at worst, with a trifling induration of the inguinal glands. One excellent gentleman I have particularly in view ; because his life being infinitely valuable to a numerous family, the case gave me more than ordinary anxiety.—I found it necessary to subject him to the influence of mercury, twice or thrice annually, from my first arrival in Antigua, for attacks of this kind, placing his life in jeopardy by their sudden transition from the groin to the head or abdomen : But, up to the time of my leaving the island, he nevertheless remained exempt, during the intervals, from the usual indications of this unlucky diathesis ; and, what is more, without either *nodes* or *bone-aches*. This must be deemed peculiarly fortunate in the case of a married man ; as, otherwise, in-

jurious doubts might have arisen, whether occurrences so suspicious were to be attributed to me, or not rather to indiscretions of his own.

Before leaving this subject, it may be proper to remark, that, after experience had given me confidence, I was induced, when either of the great cavities was implicated, to have recourse to the lancet more boldly than was sanctioned by some of my brethren, whose opinions I respected; their objection being founded upon the idea, that the inflammation was *erysipelatous*, and therefore prone to assume under depletion a low gangrenous character. But, to say truth, even admitting the force of an objection, which is now, I believe, but little regarded, I am by no means quite sure that the phlogosis in question bears the supposed analogy to erysipelas. At all events, the redness differs little from that of phlegmon, and suppuration under the form of genuine abscess is an extremely common occurrence; while, on the other hand, external gangrene is rarely, and vesications are almost never observed.

Next in the order I have adopted comes the *Dry Belly-ache* of our islands, or that peculiar form of colic for which I have proposed to substitute the more descriptive term of *Hepatic Ileus*. But, for the results of my experience in this disease, I must refer you to a paper of mine already before the profession, in which many points are considered, which bear closely upon the subject of this letter.*

I must not, however, omit to mention, as highly illustrative of what I am anxious to prove, the case of an old pupil of mine, who, under my own management, was salivated seven times within the years 1822 and 1823, for as many alarming attacks of this complaint;—yet I apprehend it would sorely amaze that gentleman, as it most unquestionably would me, were he to find himself, on that account, losing his nose or his palate,—suffering under cutaneous ulcerations, deep and foul—or graced with the corona veneris.

Nor can I help noticing the singular corroboration of my views, with regard to the efficacy of mercury in dry belly-ache, to be deduced from the reports of M. Laennec, at La Charité for 1825, as condensed in the same number of the *Medico-Chirurgical Review*, in which a favourable analysis of my paper also appeared. “A young man was under treatment for colica pictonum, but apparently with little success. Meanwhile an unequivocal peritonitis supervened, and the usual treatment of

* Observations on the Nature and Treatment of Dry Belly-ache, by A. Musgrave, M. D. London Medical Repository for November 1825; and *Medico-Chirurgical Review* for January 1826.

La Charité was instantly suspended; leeches were applied to the epigastrium, which gave momentary relief; large mercurial frictions were quickly employed, (half an ounce of strong mercurial ointment daily,) and, on the fifth, ptyalism arose, and continued more or less for a month. From the moment salivation appeared, every other symptom of disease disappeared, 'Mais le malade se trouva parfaitement guéri, et de la peritonite, et de la colique de plomb, dès qu'il eût commencé à saliver.' It has long been known," adds the London Reviewer, "that mercurialization arrests the colica pictonum, and 'that it is no mean antidote to peritoneal, or indeed any other visceral inflammation, whatever may be the scepticism of some practitioners who are blinded by prejudice,' or, according to M. Laennec, 'ce succes des frictions mercurielles, dans un cas aussi grave, prouve assez, combien elles meritent de confiance dans le traitement de la peritonite.'"

I anticipate the objection, that this mode of treatment comes badly recommended, when I acknowledge it to have given such transient relief, and to have been so repeatedly necessary, in the instance to which I just now adverted. But this objection would be more specious than valid. Let me therefore at once obviate it by stating, that the gentleman in question was obliged, by circumstances, to continue his residence on the estate where he had originally contracted the disease, and where his brother had previously fallen a victim to it,—that during each illness he was, by my advice, removed to St John's, and returned after recovery only to sustain another attack within a very few weeks;—but that the moment he was permanently removed to a distant part of the island, his convalescence was, as I had predicted, not only complete, but lasting. He has since been a student in Edinburgh. I had the pleasure a few months since of taking him by the hand in London; and he is now in perfect health assisting his brother, a practitioner at Kensington. This case was prominent among those to which I have elsewhere alluded, as convincing me of the influence of local causes in the production of dry belly-ache.

The application of mercury to the cure of *Liver complaints*, whether affecting the structure or functions of that organ, is so generally known, that it would but exhaust your patience, were I to do more than remark under this head, that the manner of prescribing the blue pill and inunction, in chronic derangements of the abdominal viscera in general, furnishes an example of the use of the remedy more analogous to the management of syphilitic cases than its bolder exhibition on other occasions. I shall therefore pass on to

Tetanus, another most important disease, in which, after a

greater share of experience than I wish to have again, of almost every mode of treatment which has yet been proposed, I have for some years placed considerable reliance on large doses of calomel, in combination with opium and camphor, preceded by copious blood-letting, when not forbidden by circumstances, and aided by active purgatives, and the external treatment which I shall immediately have occasion to mention. During the course of my professional career I have had the satisfaction to see numerous recoveries from the most aggravated degrees of the traumatic form of this complaint. Whether *post* or *propter*, with reference to the remedies employed, it may be competent to any man to question. But when we contemplate, without prejudice, the Herculean weapons which have been wielded in these struggles, between the physician and the disease, if we are sceptical enough to doubt their agency in the *cure*, we cannot choose but wonder that they failed to *kill*.

As a recent example of the practice to which I allude, and of its *occasional*, I would I could say its *general* success, I may sketch the case of a stout young negro, John Dickman, a slave of Captain Thomas Clark. This man, in the course of his duty as a seaman, on board a small coasting-vessel or drogher, had his right hand extensively lacerated by a haulyard drawn through it with great velocity, by the accidental falling of a heavy weight, to which the opposite end of the rope was attached. The flexor tendons were denuded and severely bruised, and the distal phalanx of the middle finger was forcibly torn away. Tetanic symptoms supervened on the 10th, and assistance was demanded on the 11th day from the accident.

The patient fell chiefly under the care of Dr Furlonge, who then practised with me, but was often seen by myself. He was blooded twice to syncope on the first day; and a blister was afterwards applied, extending from the sacrum to the occiput, and was not allowed to heal. The wound, after a free division of all partial adhesions, was dressed with equal parts of the oil of turpentine and laudanum, applied on caddis, which was frequently changed, and covered with a large warm poultice; and frictions were diligently instituted about the jaws, the neck, and epigastric region, with a strong anodyne liniment, or powdered opium, rubbed up into an ointment with lard. Active purgatives (such as the *Ol. Terebinth*, when the powers of deglutition permitted; the pulvis jalapæ comp. with croton oil, suspended in cinnamon water, or castor oil following upon an uncombined *Ëi.* of calomel) were also occasionally interposed and quickened when necessary by stimulating glysters. But the prescription which was assiduously persevered in, from the 6th to the 15th of the month, (or for ten days

inclusive,) was $\mathfrak{z}\text{i}$. of calomel, $\mathfrak{z}\text{ii}$. of camphor, and $\mathfrak{z}\text{i}$. of opium, with the due proportion of conserv. ros. divided into six boluses, of which he took one at various intervals, ranging from one to four hours, according to the urgency of the spasms, till he had consumed the amount of *ten* such formulæ; and, adding the quantity given with the purgative medicines, I find, on consulting my notes, that 670 grains of calomel were prescribed for this man, within the short period I have specified. Some allowance must doubtless be made for waste in administering the boluses, in a case where the difficulty of swallowing was sometimes as alarming as inconvenient, from the violent spasms which convulsed the muscles about the throat. But a more resolute fellow I have never had occasion to treat. I have seen him pause over a dose till he was enabled, as it were, to take the spasms by surprise, and then suddenly gulp the bolus with success; and in this way an enormous accumulation must, after every deduction, have taken place in the system. Yet, it is very remarkable that, while reckoning at best upon a frightful ptyalism as soon as the constitution had resumed its obedience to ordinary impressions, I was both delighted, and beyond measure surprised to find, that perfect recovery was established, without even a swelling of the gums.

I cannot pretend satisfactorily to account for an exemption which, to a similar extent, I have never observed on any other occasion. But let me suggest by the way, that by far the greatest accumulation of mercury took place during the first four or five days. The intervals between the boluses are noted after that period as considerably lengthened by the interposition of one or more doses of laudanum; and when the mercury was entirely omitted, it was deemed advisable to continue the laudanum for several days, so as gradually, not abruptly, to withdraw its effects. Is it not possible, therefore, that the susceptibility of the system might have been thus kept under control, till, by channels, remote from the salivary glands, all remains of the mercury had been completely eliminated?

As the judicious interposition of the lancet renders the system more obedient to the influence of mercury, so undoubtedly does mercury, in combination with antimony and opium, obviate the necessity of recurring again and again to the lancet; and hence, by skilful management, we can secure to ourselves the co-operation of "the right and left hands of medicine," so that they may be made mutually to spare each other, by giving and receiving assistance in opposing the ravages of acute disease. This is indeed an advantage of the highest importance, for I know of no practice more fraught with peril to the patient than that of *reiterated* bleedings. No man who has been accustomed to

meet me at the bedside will be disposed to question my firm reliance on the salutary powers of venesection, when prescribed at proper stages.—Few, perhaps, have drawn larger quantities of blood than I have within a given period. Yet I unhesitatingly express my conviction, that in enteritis, pneumonia, and (with reference to this last point) I may add apoplexy and injuries of the head, many an individual has been hurried to the grave merely because the student is taught that blood-letting is the *sine qua non*, the *unicum remedium* on these occasions, and therefore, on entering upon practice, knows not what else to do. Surely, after having emptied the vessels to a reasonable extent without success, we can at worst only lose our patient by giving nature a little play, whereas, by farther interference, we shall too probably be at once deprived of all chance from her *vis medicatrix*, which ought never to be held in contempt. Several times has an unlucky third or fourth bleeding appeared to me to accelerate and enfeeble a tolerably steady pulse to a degree from which it has gradually proceeded to entire extinction; and, although the effects of this kind of exhaustion have presented themselves to me under a form somewhat different from that which has been described by Dr Hall,* I am still gratified to observe that the attention of the profession is likely to be more pointedly turned, by his remarks, towards a species of danger which has been hitherto not sufficiently appreciated.

But I have dilated too largely upon the diseases which have already passed under review, and it therefore becomes necessary that I now bring the list to a close, by comprising in few words, that in *dysentery*, *rheumatism* of every kind, and all *acute inflammations*, whether of the head, chest, or abdomen, which assume a menacing character, calomel and opium, with emetic tartar or camphor, or both, are very commonly relied upon after, or in conjunction with, bloodletting, purgatives, and blisters; and that the tropical practitioner, who is unacquainted with the influence of alterative doses of mercury over those *ill-conditioned sores* (not venereal) to which the slaves are occasionally liable, is ignorant of the most effectual means of affording relief to a very important class of his patients. With such doses, and a regulated diet, these ulcers will yield to the simplest dressings;—without them the *pharmacopœia chirurgica* may be exhausted in vain.

I believe you are aware that in my part of the world we have no professional distinctions. We are all of that class which, in Great Britain, would be denominated *general practitioners*, that is to say, we prescribe, we amputate, or have re-

* Medico-Chirurgical Transactions, Vol. xiii.

course to the forceps or crotchet as occasion may require; with this disadvantage, however, that, in obscure or difficult cases, we are without appeal to those who have exclusively devoted themselves to either branch of practice. This state of things obliges each member of the profession to keep an establishment for dispensing his own prescriptions; and it hence occurred to me, as soon as my thoughts had been turned to this subject, that I might render the weight of my individual testimony respecting the unmixed effects of mercury on the system more definitely appreciable, by ascertaining the quantity of its different preparations actually consumed by my own patients, or those of my partners or assistants under my immediate inspection, from the time of my first embarking in practice to the present day. I accordingly wrote to our druggists, Messrs Gordon and Graham of Oxford Court, Cannon Street, London, with whom I have corresponded during the whole period, requesting a reference to their books, and a memorandum of the amount of their shipments to Daniell and Musgrave, to myself, and to the existing firm of Musgrave and Crichton; and, by their letter of the 10th of April, I find that, from the latter end of 1814 to the year 1826, they have actually supplied, to these several addresses, 111 pounds of calomel, $61\frac{1}{2}$ of blue pill, and 131 of mercurial ointment; the average consumption of the last four exceeding, as might have been anticipated, that of the eight preceding years; being about twelve pounds annually of calomel alone. It must be remembered, besides, that my experience has been by no means confined to the quantity dispensed in my own establishment, for I have always been a good deal engaged in the cases of others; and, indeed, for many years, the consultations throughout the island have been almost entirely divided between my friend and partner Dr Crichton and myself, both before and since the junction of our interests, which took place in January 1825.*

Looking, then, upon the formidable catalogue which I have thought it advisable to submit for consideration, with the evidence now adduced of the quantities of mercury I have seen expended in treating the diseases which this catalogue embraces, can you hesitate to concede to me a right which you would doubtless, in my place, claim for yourselves,—that, namely,

* I am not insensible to the danger I incur, by publishing this statement of particulars, of being regarded, by the less charitable among my brethren, as destitute, either of *principle*, in grossly mistating facts, or of *judgment*, in sanctioning such abuse of a remedy which, even by my own showing, is open to numerous and heavy objections. I must therefore acquit myself of either imputation, by observing that, independently of private practice, the medical care of a very large number of slaves attached to estates, as well as of the Colonial Hospital, and poor of the most populous town and division of the colony, gave me, from the very commencement of my career, the superintendence of a greater proportion of sick than can fall to the lot of any but those in charge of large hospitals in this part of the world.

of inferring,—if not the absolute *impossibility*,—at least the extreme, the almost inconceivable *rarity* of occurrences which could altogether have escaped the most watchful observation, during the period, and under the circumstances to which this letter refers? And assuming a concession which cannot, I apprehend, be consistently withheld, may I not confidently go farther, and ask of those who believe in the pernicious influence of mercury on the bones and their periosteum, the throat, and the skin, how this belief is to be squared with the assertion which I now unequivocally make,—in terms and by facts which court publicity, in order that they may be rebutted if possible by the testimony of others,—that I have never either seen or heard of a case where symptoms, bearing the stamp of secondary syphilis, in the common acceptation of these words, were even *suspected* to be the offspring of mercury administered for the cure of any other disease,—that the *only bones* I have ever seen, either immediately or ultimately, affected by the most aggravated cases of mercurial mismanagement, were those of the upper and lower jaw, whose vitality might have been partially destroyed by a process of denudation, caused by extensive sloughing within the mouth, which involved their periosteum,—and that the sole injury I have had occasion to notice after recovery from excessive ptyalism, which could even *by supposition* be connected with any but the two bones I have mentioned, was on one occasion a paralysis of the *portio dura* of the right side, producing that peculiar effect upon the countenance, so well described by Mr Bell? This paralysis, however, evidently arose from the thickening of some portion (probably the lining) of the canal, through which that nerve passes from within the cranium to the face, caused by inflammation extending to it by *direct continuity* of parts.*

* “Dr Thomson (Henry) has served in hot climates, and he acknowledges that the affections of the bones, attributed to mercury, ‘very seldom take place;’ but that they *never* happen ‘he is *convinced* is not the case.’ Now, to this we would reply in the words of the lawyer in the play—‘No *convictions*, Sir; stick to *facts*.’ Dr Thomson does not produce a single fact, nor does he assert that he ever saw a case of the kind. He is *convinced*, however, that they do occur, because ‘it is consonant to *reason* that they should.’ Again, we say ‘no reasoning, stick to facts.’ For our parts, we have seen at least *some thousands* who had undergone courses of mercury for dysentery, hepatitis, fevers, &c. and yet we cannot charge our memory with a *single* instance in which diseases of the bones were the consequence.”—See Johnson’s *Med. Chir. Review*, Vol. ii. p. 621.

“It were easy for me to refer to numerous cases similar to the above, but at present this appears to be totally unnecessary; for while rotten bones have long been considered as the legal inheritance of those who have suffered much from venereal disease, I am not aware that a *single instance* has yet been brought forward of mercury producing a similar affection of the bones, when exhibited for the cure of any other disease than syphilis; and until well-marked and unequivocal cases of this kind shall be produced, those who advocate the utility and safety of mercury are not bound to obviate an objection to its use, which has not been proved to exist.”

Remarks on a Syphilitic Cranium by George Ballingall, M. D. F. R. S. E. Regius

Having thus proved, as clearly as the nature of the question admits, that, from the numerous objections which may fairly be urged against an abuse of mercurial medicines, we must exclude all apprehension of constitutional injury to the bones, under any circumstances whatever, when the venereal virus does not contribute to that injury; I must now endeavour to convince you and your readers, that the latter is abundantly capable, without foreign assistance, of giving rise to those extreme cases, which have been the chief source of discordance among medical men; for if it be not so, then am I yet to learn how several which I have met with among the slaves are to be rationally explained.

The promiscuous intercourse indulged in by our black population is, I believe, sufficiently notorious, nor am I sanguine in hoping that they will be speedily prevailed upon to lead more regular and creditable lives.* But the younger women have, nevertheless, a certain character to support. Many of them are ambitious of forming temporary connexions beyond the class in which they live, not improbably with the white persons on their own or the neighbouring estates; and hence it is not difficult to appreciate the reluctance which they must naturally feel to expose themselves to neglect or disgrace, by applying at the public hospital or sick-house for relief. The consequence is, that they consult nobody till matters are too far advanced to be longer concealed from those more immediately about them. Even then, however, with the entire concurrence of the seniors of the family, the requisite assistance is sought for, not from the medical attendant of the master, but at the hands of some black and veteran *Æsculapius* on the property; for we abound in such characters, both male and female, eager to undertake by *bushes* the cure of complaints, which they contend are less successfully managed by the licensed prac-

Prof. of Military Surgery, &c. &c. &c.—Edinburgh Medical and Chirurgical Transactions, Vol. i.

Here then we have the testimony of two practitioners from the *East* (for I presume the reviewer first quoted to be Dr Johnson himself) added to mine from the *West*, on a point, regarding which it is really difficult to conceive how a doubt could possibly have arisen.

* I must not, however, deny myself the gratification of stating, that the most active arrangements are now in progress, through the zealous influence of our excellent and distinguished Bishop, (independently of the valuable labours of the Moravian missionaries, who receive an annual stipend from the colonial treasury) for spreading moral and religious instruction among the slaves; it is therefore to be hoped that a reformation in this respect, however slowly it may proceed on account of the inveteracy of habit, will at length be effected; more particularly as every encouragement is offered by the proprietors themselves to fidelity in their connexions. The morals of the next class, the coloured inhabitants, have been wonderfully improved within the last fifteen years—an improvement which must, in candour, be ascribed chiefly to the laudable exertions of the Methodists, who have hitherto conducted themselves, in Antigua, with marked propriety.

titioner. The necessity this concealment enforces of laborious exposure to the vicissitudes of weather, added to the continued use of their favourite diet, into which salted meats and fish always enter, as largely as their means will allow, produces a daily augmentation of evil, till at length the poor woman presented for treatment, sometimes with ragged ulceration of the throat, accompanied by caries of the bones of the palate and nose; at others with nodes and bone-aches, cutaneous blotches; or, to be brief, with any of the infinite variety of appearances which it would be superfluous to enumerate here.

It will readily be granted, on my part, that the imperfect history almost always obtained with cases of this kind, in which we are often gravely assured that the unhappy girl is yet a stranger to sexual intercourse, precludes our arriving at absolute certainty that no mercury has been previously used; but when we reflect upon the powerful motives which have prompted concealment—the difficulty of obtaining the necessary medicines, even if these people knew how to use them—and the fact, that the sore mouth is the principal object of dislike, it appears to me to be the extreme at once of credulity and scepticism, to suppose that mercury could, by the most remote possibility, have contributed to the existing symptoms.

To the evidence previously adduced of the impotency of mercury in this respect, I have now added strong testimony of the intrinsic power of syphilis to generate the injuries in question to an unlimited extent; but I must not yet quit the subject without giving the outline of a case, which, as far as one case can be permitted to guide our opinions, places beyond all cavil the conspicuous effect of the former in arresting the most threatening assemblage of symptoms, when administered with proper precautions.

A gentleman resident in London having had occasion a few years since to visit the West Indies on business of consequence, proceeded to Falmouth to join the packet. There he had connexion with a chamber-maid; and the product of that connexion was a chancre, discovered during the first week of the voyage. He was an elderly and a married man.—Even the kind of duty in which he was engaged rendered his moral reputation a matter of the last importance; and more especially, as he had heard something of the new opinions regarding syphilis, he determined resolutely to keep his own counsel. He dressed the chancre with cold cream and lint; and having on board with him six calomel pills of a single grain each, which some sapient adviser had directed him to take on his approach to the tropics, as a preparative for the climate, he at once consumed them, at intervals of a day or two between each; without deeming it necessary, however, to abstain from the good cheer which was

daily provided for the table. The chancre healed kindly enough, but not before a bubo had appeared in the groin, which he permitted to take its course. It suppurated, and, before he reached Barbadoes, not only broke, but assumed an angry appearance. He was now a miserable man, and mental distress conspired with bodily annoyance to injure his general health. He became sallow and emaciated. The sore gradually got worse; and yet the dread of exposure kept him obstinately silent, till six weeks afterwards, when he arrived in Antigua, and applied to me in despair.

I shall not readily forget how shocked I was when the groin was submitted to my inspection. It presented a phagedenic bubo of the most alarming description, the ulceration being exceedingly deep, and spreading from the whole extent of Poupart's ligament, upwards to the old spot for tapping in ascites, and down the thigh about two inches and a half. The gentleman's strength was materially reduced, and I was literally at a loss in what manner to proceed. The following plan, however, was, after due deliberation, adopted.

In the first place I endeavoured so to regulate his diet as to meet his debilitated condition without fomenting the mischief. Half a drachm of strong mercurial ointment was then directed to be rubbed into the thighs thrice a-day, and five grains of blue pill, with a full dose of opium, to be taken every night at bedtime. The sore was carefully bathed at proper intervals with the sol. sulph. cupri. comp. diluted according to the sensation produced. Caddis dipt in the same was laid on it, and the whole covered with an emollient poultice. The bowels proved so irritable that the blue pill was occasionally suspended,—but I need not follow up the minutiae of the case.—Suffice it to say, that the requisite caution caused the saturation of the system to be more tardy than was desirable, and on the sixth day from my first visit, not only had no improvement on the sore been made manifest, but incipient ulceration was discovered in the throat. This, as may be imagined, rendered me doubly anxious to arrest the progress of the symptoms, and I accordingly gave, but not without occasional hesitation, a second pill daily, and increased the original quantity of ointment to ℥ ii. at each friction. On the fourteenth day, however, the mercurial fetor betrayed itself—the gums began to swell—and it is no exaggeration to assert, that from that moment the aspect of affairs was entirely changed. The throat healed rapidly;—the circumference and depth of the horrid ulcer I have described were daily lessened to a degree which delighted the patient and astonished me;—and in three months from his landing in Antigua he again embarked, after having gradually regained flesh, strength, and spirits, with no trace of his misfortune but an ugly cicatrix in

the groin. The circumstances of this case must speak for themselves.—They require no comment from me, except this, that the manner in which I came by the occurrences, which preceded my personal interference, precludes the most distant suspicion of anything like want of candour, in the detail which I received from my patient.

Here then are "*factis contraria facta*." I cannot, of course, doubt the accuracy of my own assertions, and I distinctly profess an equal conviction of the correctness of every thing advanced, as the result of observation, on the other side. But the discrepancy is more apparent than real when the *facts* are considered. It is in the *conclusions* alone to be deduced from these facts that I am in any manner opposed to the prevailing opinions. The comparative mildness, for example, of the secondary symptoms, after the non-mercurial treatment, on which every thing hinges, (which symptoms, however, are admitted to have shown themselves in a modified form under the very best management,) I have no hesitation in ascribing to the antiphlogistic regimen, aided by confinement and posture, which being so strenuously enjoined was, doubtless on these occasions, rigidly enforced. On the other hand, it does not require preconceived notions to convince me that the complicated ravages which so frequently present themselves, after reiterated courses of mercury, are the result of that miserable practice which still too generally obtains upon Mr Hunter's authority, of allowing full diet and wine, while the patient makes no sacrifice of business, pleasure, or exercise, during the cure of the primary symptoms. When we look back indeed upon the number of cases, in which this system has most probably been pursued, it becomes matter of wonder, not that bad consequences are occasionally met with, but that they have failed to occur in a tenfold ratio; and were I at this moment compelled to select between mercury with free living, and the antiphlogistic regimen with rest, as the irrevocable plan on which I was in future to proceed, I should have little difficulty in deciding upon the latter; but I should do so as the forced choice of two evils, being thoroughly satisfied that a judicious combination of mercury, with diet, confinement, and posture, will best secure that uniform and permanent success which alone can be satisfactory either to my patients or to myself.

In establishing the positions to which I pledged myself at my outset, it only remains for me to refer to the conclusive evidence extracted by Dr Mason Good,* from the official records to which he had access by permission of the Director-General,

* Study of Medicine, Vol. iii. 394.

in proof of the extraordinary hebetude, as he calls it, of the syphilitic action in the West Indies, or, in other words, of the comparative infrequency of venereal cases, and particularly of those under a secondary form. The number indeed of these cases, which appear in the annual returns to have been afforded by the troops throughout the Islands for a series of years, falls short of what my own experience would have led me to suppose ; but the inference drawn by Dr Good is to a certain extent decidedly corroborated by general observation, and I must therefore leave it to acumen more pointed than I can pretend to, to explain away the facts I have adduced, and to reconcile this hebetude of action with the idea, that the unmixed effects of mercury and reputed syphilis bear the slightest resemblance or affinity to each other.

When commencing this communication, I fully intended to enter more at large upon the injurious consequences which really result from the improper use of mercury, than I shall now be able to attempt, on account of the length to which these desultory remarks have already extended. I must therefore confine myself in this place to briefly stating, that these consequences are sufficiently serious to render every prudent man circumspect in the exhibition of so active a remedy.

I am far, very far, from desiring to conceal that the immediate effects of mercury, are now and then appalling.—I am only anxious it be at the same time remembered, that these effects are unmasked and *characteristic*, not insidious and lurking. I frankly acknowledge, so shocked and disgusted have I twice or thrice felt myself at witnessing the sufferings of a victim to the outrageous salivations which are occasionally brought on, that I have been tempted, under the impulse of the moment, to resolve that I would abandon the use of calomel for ever.—But to this acknowledgment must be added, that five minutes reflection sufficed to remind me that the example before me was but *one* opposed to one *thousand*,—that I must not disregard the overwhelming balance which daily experience enabled me to note on the other side. To conclude,—while I hold a resolution not hastily framed, and therefore commended to general adoption, never, without due consideration, to risk the super-vention of ptyalism, and, when saturation of the system is really demanded, to proceed with measured doses, at regulated intervals, to as limited an accumulation of the medicine as the urgency of the danger admits ; I am yet so conscientiously persuaded of the unrivalled power of this treatment in many of the most fatal diseases, that I must continue to prescribe it fearlessly, even at the peril of my reputation and income ; and should I, on that account, be deprived of the confidence of those

around me, I must be content to retire from the exercise of a profession, which demands of me the sacrifice of duty at the debasing shrine of self interest.*

I have now, I believe, amply redeemed the pledge with which I prefaced these remarks, and redeemed it by an appeal to facts and arguments, which, being irrefragable, will, I trust, hold me acquitted of presumption in venturing to treat so confidently of these subjects. I am indeed ashamed to acknowledge the extent of the opportunities I have enjoyed for professional observation, when the glaring disproportion of my attainments can be so readily detected; and, had it been practicable, would gladly have confided this matter to an abler and more experienced pen. But my views are too unpopular in Edinburgh to be advocated by others, and I have therefore felt it,—I repeat—to be an act of indispensable duty, in return for the indulgent share of confidence awarded me where I practise, to raise my voice against the farther promulgation of doctrines, which have already gone too far in deterring the rising members of the profession from availing themselves of a remedy, which I firmly believe to have been in my hands, under Providence, the means of saving many an estimable life, and which the records of medicine, for the last thirty years, abundantly prove to have been equally successful under the management of others. In expressing myself thus earnestly, I have the satisfaction to reflect, that I stand personally absolved from all sinister motives; for, to vindicate as warmly as I have done the extensive, though circumspect use of mercury, would, even in our climate, be enough to blast the prospects of a man who had yet his character to establish. I have already adverted at some length to this subject in my paper on dry belly-ache; but it may be well to repeat, that it requires a degree of moral courage—of strict and unbending principle which all do not possess,—to enable us to persevere in the use of a remedy, however valuable, whose babbling effects, even in successful cases, will injure our reputation more than the last and worst result of ignorance or mistake. In the one case the coffin is consigned to the grave, and

* “I have found, in the management of the mercurial treatment, a small quantity of mercury sufficient to excite salivation, and to secure safety; and I have known 6000 grains (including the frictions) required to effect the same object. Such is the uncertainty on one hand; such the necessity of the precept I have recommended on the other.”

“Are we then, from any vain or unfounded apprehension of this kind; from reasoning drawn from false premises; or from the suggestions of uninformed or prejudiced minds, to yield up the result of our own frequently reiterated experience—to relinquish the best aid we can bring to the support and relief of our fellow-creatures suffering under direful maladies? Forbid it, humanity! forbid it, truth! forbid it, Heaven!” Chisholm, *Manual of Tropical Diseases*, 1822.

the man, his treatment, and his death, are speedily forgotten. In the other he lives, but either doubts or forgets the extent of his danger. While a trace of ptyalism remains, he dwells pathetically to his friends on the cruel sufferings he so needlessly endures,—and the loss of one useless tooth perpetuates our mortification and disgrace.*

It is indeed high time that the public be made aware how utterly groundless and untenable these prejudices are found in reality to be, when tried by that scale of experience which alone can be regarded as the test of truth. I am, Gentlemen, your faithful and obedient servant,

A. MUSGRAVE.

31, Howard Place, Edinburgh,
May 8th 1827.

ART. VI.—*An Outline of the Homœopathic Doctrine, or the Medical Theory of Hahnemann.* By EDWARD JOHN SPRY, London.

“Lerne, so viel es in deinen Kräften steht, alle vorzüglichern Systeme kennen, lasse dich aber von keinem derselben je beherrschen.”

THE Homœopathy has excited as much interest in Germany, and led to as much angry discussion, as the doctrines of BROUSSAIS and RASORI have occasioned in France and Italy; but, from various circumstances, it has been little known in the west of Europe, and as yet, I believe, no account of it has been published in this country. It would be surprising indeed if any system, however absurd in the mass, did not contain some truth in its details, and it will probably be found that some of the opinions of Hahnemann, viewed abstractedly from the theory into which they have been twisted, are worthy of attention; at any rate, the relation of them may gratify those who are interested in the progress of medical history.

The following description of the homœopathy is only, what it professes to be, an outline of the doctrine, as well as I could understand it during my residence in the country, and is an abstract of a paper which I had the honour to read at one of the recent meetings of the “London Medical Society.” It must be at the same time understood that I appear only as the narrator, not as the advocate of homœopathical opinions.

According to HAHNEMANN, in order to cure diseases speedi-

* Need I point out with what additional force these remarks apply to cases in which ladies are concerned?

ly, we must have recourse to such remedies as are capable of producing a train of symptoms similar to those to be removed ; he therefore adopts as an axiom “ *similia similibus curantur.*” This doctrine he first broached about thirty years ago in some of the early volumes of Hufeland’s Journal, in a series of articles, entitled “ An attempt to discover, on a new principle, the healing power of medicinal substances ;” * and some time afterwards having digested his reasonings into something like an original theory, he chose to give it the name of *homœopathy*, derived from *ὁμοιον* and *πάθος*, a similar disease. It was the chief object of these papers to show, that the active properties of most remedies were very imperfectly understood, and that the only accurate mode of investigation was to try their effects on men in health, and carefully to note every symptom produced by their use. He endeavoured to show that this was the only way by which we could arrive at precision in their application in the diseased state, or ever become possessed of a true materia medica. He recommended physicians to try these experiments on their own persons, and, from repeated trials on himself, was induced to conclude, that all medicines, when administered to persons in health, produce a temporary or artificial disease ; that by exciting such an artificial disease, the one previously existing might be removed, and that speedily, or otherwise in exact ratio to the degree of similarity existing between the old and new series of symptoms. He contended, moreover, that, by perseverance, it would be possible to discover a sufficient number of antidotes, not only to equal the number of diseases at present known, but even considerably to surpass them, if proper diligence were employed in their search. In short, that, in order to remove any disease, arising either from physical or moral causes, it would only be necessary to set up its prototype. In support of this reasoning, he endeavoured to show that this is the method employed by nature in what are generally called spontaneous cases of recovery ; that the practices of unprofessional persons, of the ancient physicians, and of many of the moderns, confirm its accuracy ; and that they have been from experience, although unconsciously, *practical homœopaths*.

The peculiarities of the proposed maxims appear to have attracted general attention ; they were supported by some, and ridiculed by others ; and as no schemes are too visionary not to procure some proselytes, Hahnemann succeeded in raising a party who publicly espoused his opinions. A long paper war was the consequence, which has not yet terminated, but which

* Versuch über ein neues Prinzip zur Auflindung der Heilkräfte der Arzneisubstanzen.—Journal der Practischen Heilkunde, B. i. and ii.

has succeeded in setting the medical men of Germany by the ears, and of forming them into two great sects, the *Homœopathetikers* and the *Antipathetikers*.

In 1810, Hahnemann published his "*Organon der Heilkunde*," containing a systematic account of his opinions; in 1808, a second edition of the same work made its appearance, which was translated into French by Count BRUNNOW of Dresden;* and in 1824 a third edition was produced, from which the present description of the doctrine is principally taken. Besides these three editions of the *Organon*, he has written a large octavo work in six volumes, entitled the "*Pure Materia Medica*."† As the cause gained strength, it was deemed necessary to establish a journal, to keep good the ground that had been gained, and, like Broussais, Hahnemann established his "*Archives*," which were published at Leipzig, and said to be conducted by an "*Association of Physicians*."‡ Five volumes of these have already made their appearance. CASPARI and HARTLAUB have appeared as the defenders of the doctrine; and JORG WEDEKIND, LICHTENSTADT, HEINROTH, NAUMANN and SACHS, have been its principal opponents. These are only a few of the rival disputants; the anonymous productions in the shape of catechisms, squibs, and lampoons, in favour of the Homöopathical party and against it, are too numerous to give even a list of their names. HEINROTH, professor of physic at Leipzig, has undertaken a serious refutation of the doctrine, in a work called the "*Anti-Organon*;"§ written with more temper than is displayed by many of his colleagues, and has successfully exposed much of its absurdities. In the "*Litterarische Annalen der gesammten Heilkunde*,"¶ very ably conducted by Professor HECKER of Berlin, there is a good critique or exposé of the Homöopathy by Lichtenstädt. The latest opposition monograph with which I am acquainted is from the pen of SACHS, professor of medicine in the University of Königsberg: it was published toward the close of the last year, and, like many other authors, considering his arguments too conclusive to be refuted, he thinks he has given a death blow to the Homöopathetikers.** Dr SCHOENBERG of Naples has written

* *Organon de l'art de guérir*; traduit de l'original Allemand du Dr S. Hahnemann. 8vo. Arnold, Dresden, 1824.

† *Die reine Arzneimittellehre*. Dresden, 1811 and 1821. This work has also reached a second edition, the last part of which was published in 1825.

‡ *Archiv für die Homöopathische Heilkunst*: commenced in 1822, still continued.

§ *Anti-Organon oder das Irrige der Hahnemannischen Lehre in Organon der Heilkunst*, dargestellt von Dr Joh. C. A. Heinroth. Leipzig, Hartman, 1825.

¶ In the September and October numbers of 1825.

** It is entitled "*Versuch zu einem Schlussworte über Hahnemanns Homöopathisches System*." Leipzig, 1826.

a very voluminous exposition of the doctrine,* which has been extensively circulated; and I have been informed by my friend Mr Loudon, who has recently returned from Italy, and who had the goodness to show me a copy, that many physicians in that country, but more particularly at Naples and its neighbourhood, have become converts to the theory. In Vienna and Prague it is becoming quite fashionable. So that it is pretty evident from this imperfect account of the history of the Homöopathy, that, whatever may be its merits, it has excited considerable discussion among the profession throughout Germany, and has appeared to many of sufficient value to enter the lists in its defence. HUFELAND, the senior professor of physic in the University of Berlin, has negatively patronized the doctrine, by inserting the cases treated by the Homöopaths in his Journal, and he thinks that the theory has had a beneficial effect upon the practice of his countrymen, by leading to a more precise application of remedies, and to a stricter attention to diet. He speaks in the highest terms of Hahnemann's professional character, and considers him one of the most distinguished, spirited, and original physicians of our times. It is but justice however to state, that I received a very different account from many very respectable practitioners; and I was informed of several circumstances which partook too much of quackery and imposition. For a long time he prepared his own medicines to prevent their being known, and was eventually expelled from Leipzig for thus encroaching on the privileges of the apothecaries, and for persisting to keep his remedies a secret. He now resides in the Duchy of Anhalt-Coethen, situated between Magdeburg and the Saxon frontier, where he carries on a very extensive practice, and is frequently consulted by persons who come from a great distance for that purpose.

It is curious to observe, that the first cases which he has chosen in confirmation of the success of the Homöopathic practice are taken from the records of the history of medicine in this country; and he endeavours to show, that even Hippocrates† himself was a Homöopathic practitioner. "Men," says Hahnemann, "have been heretofore accustomed to cure diseases after a miserable fashion, by purely fantastical modes of procedure;

* Il sistema medico del Dottor S. Hahnemann esposto alla Reale Accademia delle scienze di Napoli, dal Dr J. J. Schoenberg. Napoli, 1822.

† "He imagines that this was the case from a passage in the little book *περὶ τῶν πλεθρῶν καὶ ἀνθραξῶν* which he quotes in the Organon from the seventy-second page of the Basil edition; but, as it is not fairly given, this support falls to the ground: it is, however, very plain, that the father of physic was opposed to such a doctrine, if we may judge from the second section of his twenty-second aphorism. "Diseases from plethora evacuation cures, as those from depletion, supply, and so with all, *always the contrary*."

and, among others, have adopted the merely palliative rule of ‘*contraria contrariis* ;’ but the true and natural method is the following, and it may be comprised in these words,—*in every case, to cure a disease mildly, speedily, certainly, and permanently, choose a medicine capable of producing an affection similar to the one to be removed.*” He then goes on to state, that the histories of cases in which the remedies were exhibited in a simple form, so that their effects could be judged of, support this assertion ; that in all such cases it will be seen that the disease was removed by a medicine which produced a similar, and not a dissimilar disease, as the prescriber supposed. The English sweating fever of 1485, which, according to Willis, proved almost as destructive as a plague, and which continued its ravages until powerful diaphoretics were administered, is referred to by Hahnemann by way of example. He appeals to the practice of rubbing frost-bitten parts with snow and the holding of scalded parts to the fire ; and quotes the authority of Mr John Hunter, as confirming the utility of such treatment. Dr Edward Kentish’s mode of treating the men burnt in coal pits, by rubbing them with hot oil of turpentine, is likewise appealed to. The case related by Mr John Bell, of a lady who had scalded both arms, who plunged the one into cold water, and rubbed the other with oil of turpentine, is cited as a convincing proof of the great superiority of the latter, or Homöopathic practice. Having quoted many similar cases from Heister, Sydenham and others, he exultingly asks, who can fail to acknowledge the decided preference that should be given to the Homöopathic mode of cure ; and who cannot see, in comparison with it, the miserable insufficiency of the old mode, the “*contraria contrariis*” system ? There are some circumstances, however, to which he refers, that appear to give some sort of support to his opinions, as the removal or the prevention of the small-pox by the cow-pox, and the cure of the venereal disease by mercury ; the constitutional symptoms produced by the two last being frequently so similar as to prevent men of considerable experience from discriminating them accurately.

Having, as he considers, established this as the first axiom, he proceeds to another, which is expressed in the following words. “ Disease, for the physician, may be said to exist only in the *totality of its symptoms*, and it is only necessary to remove this totality of symptoms, in order to cure the disease ; but this can only be done by such medicines as have the power of producing similar, and to a certain extent stronger symptoms. This knowledge of the powers of different substances can only be learnt by experiments on healthy men.” He then endeavours to show that such is the process of nature, that nature can cure

diseases only by setting up others of similar symptoms to the originally existing disease. According to Hahnemann, there are only three ways of curing a disease; 1st, the *Allopathic*, or the mode of transferring it from one part to another; 2dly, the *Homöopathic*, which he asserts is the only efficient method; 3dly, the *Antipathic*, palliative method, or "*contraria contrariis*" system, the worst of all.

He contends, that the last is the worst method, because a single symptom of the disease is combated by a medicine producing an opposite symptom, to the neglect of the others; "the consequence is, says Hahnemann, that, although the patient may appear at first a little benefited, a worse state always supervenes, and this is in a great measure owing to the immense doses usually given in the palliative mode of cure. Such large doses produce, what no other person has before noticed, a great secondary effect on the constitution, an injurious reaction." He lays it down as a general law, that all medicines produce two distinct effects on the system, a primary action (*Erstwirkung*), and a secondary action, (*Nachwirkung*.) As examples of these distinct effects of the same remedial agent, he mentions among others the following; a hand plunged into hot water is at first much warmer, but when taken out and dried soon becomes much colder than the other. After great bodily exercise a man feels very hot (*Erstwirkung*); but after a time cold and shivering follow (*Nachwirkung*.) He considers that the secondary effect of medicines is always injurious, and that it is caused by the largeness of the doses usually employed. To avoid this secondary action, he prescribes his medicines in exceedingly minute doses, by which, he says, the immediate or primary action of the remedy succeeds in bringing back the patient to the equilibrium of health. This is the Homöopathic mode of procedure, by which, with suitable moderate doses of medicine, symptoms the most resembling the symptoms of the natural disease are to be produced; and this, says Hahnemann, is the only way in which diseases are to be perfectly and permanently healed.

As the causes which produce disease are almost innumerable, and the modifications in the condition of the body when suffering from its influence equally so, the labour imposed upon the Homöopathics must be very considerable; since it is laid down by Hahnemann, that every medicine has its peculiar operation, and that there are scarcely any two cases in which the same medicine can be used with success. Of the *modus operandi* of the Homöopathic treatment, the following account is to be found in sect. 155 of the *Organon*. "A medicine when selected according to the foregoing rules, and given in appropriate doses, sets

up in the suffering part of the organism its own peculiar influence, or establishes in that part an artificial, but somewhat stronger disease, which, on account of its great similarity and preponderating influence over the former disease, takes its place, and the organism from that time forth suffers only the artificial disease. This again, from the minute doses of the medicine, soon subsides of itself, and leaves the patient altogether free; that is to say, the person is permanently cured."

The substances to be chosen as medicaments are to be used in their natural and unprepared state, and Hahnemann asserts, that the best mode of preserving the active principle of plants is to mix the expressed juice with an equal quantity of pure spirit. If the plant be powdered, the greatest care should be taken to dry the substances most carefully in the dark, and after the plant has been powdered, it should undergo another process of drying, for which the heat of a water-bath is said to be the best. It should be dried until the particles of the powder, in being stirred, go abroad like fine sand; it should be preserved in closely stopt glass bottles, and carefully kept from the direct rays of the sun; if in the dark so much the better. No more than one medicine, and that in a very simple form, is to be administered in a single case.

It is not only necessary to the success of the practice that a medicine be suitably chosen and prepared; but great care must be taken to administer it in very minute quantities. If the dose be large it will certainly increase instead of diminishing the disease. In the preface to his "pure Materia Medica," Hahnemann has given the results of a great variety of experiments which he had tried on himself; and he was led to conclude, that the effects of remedies do not diminish in arithmetical progression with their diminution in dose. Eight drops of any tincture do not act with four times the efficiency of two, and so on.

The extreme minuteness of the doses which he employs appears to preclude all chance of their exerting any effect; yet he speaks very decidedly of their sufficiency. He particularly insists upon a great attention to diet, and as the cases in which he is consulted are for the most part chronic, it is probable that he is indebted for much of his success to his attention to this point. He professes to have no predilection for particular medicines, but it is pretty evident from his writings that he has some favourite preparations. The *Atropa belladonna*, the *Cannabis sativa*, the seeds of the *Menispermum cocculus*, the *Nux vomica*, *Datura stramonium*, *Arnica montana*, *Pulsatilla*, *Aconitum*, *Opium*, and *Hyoscyamus*, appear to be the most frequently selected vegetable substances. The minuteness with which he watches the effects of remedies may be

imagined from the fact, that the enumeration of the symptoms arising from the use of the *Nux vomica* occupies fifty pages of the first volume of his *Arzneimittellehre*, and those of opium, fifty-three ! According to his observations,

Camomile flowers produce	-	-	-	1481 symptoms.
Elder flowers	-	-	-	116
The flowers of <i>Verbascum</i> , or great Mullein	-	-	-	175
Iron	-	-	-	228
Bark	-	-	-	691
Platinum	-	-	-	402

He observes, that he has already discovered this number of symptoms to belong to each of the substances mentioned, but that it is probable there are many yet remaining to be found out. He mentions sixty-nine different remedies which he has tried on himself.

In the second volume of his *Materia Medica*, he gives the doses of different substances, but for brevity's sake only a few are selected. Speaking of the doses of arsenic he says, that a $\frac{1}{10000}$ part of a grain is the largest dose that should be given ; that $\frac{1}{10000}$ is enough in ordinary cases, but that sometimes a millionth, or even a four-millionth part of a grain is too much. Of the solutions of iron in muriatic, acetic, and sulphuric acids, he says, that from the thousandth to the ten thousandth part of a grain may be esteemed an average dose, but that sometimes the thirty thousandth part of a grain is too strong. Of the tincture of the seeds of the *St Ignatius* bean, the dose may be computed at about the three millionth part of a grain of the seeds ; but he mentions that he has seen very decided effects from the four millionth part of the same quantity. A drop of the spiritous tincture of *sarsaparilla* is said to be a strong dose. One hundred grains of the *Cucumis colocynthis*, dissolved in one hundred drops of spirit of wine, is a form which he particularly recommends, and it is asserted that the seven millionth and even the eighth millionth part of a drop of this solution acts sometimes too powerfully !

However ridiculous such assertions may at first sight appear, there are some facts which seem to bring them within the scope of possibility. Well authenticated cases have been recorded of syncope having been produced by the smell of rose-water, and of persons having become speedily nauseated on being introduced into a room containing a small quantity of musk. A grain of the latter substance serves to furnish a decided odour to the atmosphere of a large apartment, without losing any of its sensible weight.

It may not be uninteresting to show how such opinions ope-

rate on practice, and if we may judge from the opportunities afforded by Hahnemann himself, the conclusion cannot prove very satisfactory. The following cases are selected from Hahnemann's own works,* and were treated by himself.

Case I.—The patient was a stout washerwoman, about 40 years of age, who had been unable to work for three weeks when she applied to the doctor. The following is a literal copy of his description of the case :

Symptom I.—At every movement there is an acute pulsating pain in the cardia, particularly when the foot is put to the ground ; whenever a false step is made the pain comes on in the left side.

II.—On lying down she was quite easy ; no pain being felt anywhere.

III.—Not able to sleep after three in the morning.

IV.—After eating she had nausea.

V.—After eating there was a collection of fluid in the mouth, as if she had worms.

VI.—After she had eaten she had eructations, but without odour.

VII.—She was of a choleric temperament, and during the paroxysm of pain was covered with a sweat. All the other functions were regularly performed.

(Remarks by Hahnemann.)

“ Now as to the symptom No. I. *Belladonna*, *Cinchona*, and *Rhus toxicodendron* produce pain in the heart. *Bryonia alba* produces it also, with the additional power of causing pungent pains under the sternum on raising the arm. As to the negative symptom—No. II. there appear to be other medicines besides the *Bryonia* which have the power of tranquillizing pain on lying down ; but none so decidedly, especially in connexion with the other symptoms, as *Bryonia*. As regards No. III. that symptom may be produced by many medicines as well as *Bryonia*.—No. IV. As to the nausea after eating, several medicines produce this effect, but *Bryonia* more especially.—No. V. Several medicines produce a copious secretion of fluid in the mouth, so does *Bryonia* ; but other medicines do not produce the other symptoms detailed in the present case. Hence the *Bryonia alba* is to be preferred. The eructation after eating follows the employment of but few medicines, but it happens when *Bryonia* is used, consequently on that account also it is to be chosen. The state of the mind or temperament is also to be considered, and *Bryonia* produces such a change in the disposi-

* *Rein Arzneimittellehre*, 2ter Theil, S. 28.

tion as is mentioned in No. VII. Hence, of all medicines, the Bryonia is most applicable to the present case, if we wish to effect a Homöopathical or permanent cure." Hahnemann observes at the conclusion, that, as the woman was robust, and as her disease was so considerable as to keep her from all kind of work, he was induced to give her one of the strongest Homöopathical doses, namely, an *entire drop* of the expressed juice of the root of the Bryonia alba !

Case II.—A weak, pale-complexioned man, 42 years of age, an accountant, applied to Hahnemann five days after his complaint had appeared. The symptoms are stated in the following order :

Symptom I.—In the evening he was attacked, without any apparent cause, with vertigo, nausea, and eructation.

II.—The following morning at two o'clock he had some vomiting.

III.—The following night eructation.

IV.—Each eructation was fetid and acid.

V.—He felt as if the food remained undigested in his stomach.

VI.—He felt as if his head was empty, and was at times rather confused, but at all times retained his consciousness.

VII.—The slightest noise troubled him.

VIII.—His mind was tranquil.

(*Remarks by Hahnemann.*)

“ From these things we are to observe, 1st, That some medicines produce vertigo with nausea, particularly the Pulsatilla. which produces vertigo in the evening, a fact never observed by any other person. As to No. II. vomiting of a mucilaginous fluid, with a bad smell, is produced by the Datura stramonium and Nux vomica, but as far as is yet known not in the night time. The Valerian and Menispermum cocculus produce vomiting in the night, but not acid vomiting. Iron produces vomiting in the night, and more, it produces acid vomiting, but not the other symptoms which must be taken into consideration. The Pulsatilla produces not only evening acid vomiting and nocturnal vomiting in general, but it also produces all the other symptoms of this case, which iron does not. As to symptom III. Nocturnal eructations occur after taking the Pulsatilla.—No. IV. Fetid, putrescent and acid eructations occur after the Pulsatilla.—No. V. A sensation of indigestion is produced by no medicine so decidedly as by Pulsatilla.—No. VI. The seeds of the St Ignatius bean produce this symptom, but not the others of this case, as the Pulsatilla does.—No. VII. The Pulsatilla also produces this symptom, and besides it acts decidedly upon all the organs of sense, but principally on that of sight. As to

No. VIII. the Pulsatilla also produces a tranquil state of mind, and therefore on all these accounts the Pulsatilla is to be chosen."

On account of the weakness of this patient, and the peculiar way in which he was attacked, a very small dose only was necessary, that is to say, only the one-half of a drop of the four millionth part of a large drop of the juice of the Pulsatilla; only the *eight millionth part of a drop of Pulsatilla* was taken by the patient toward evening! On the following day Hahnemann asserts that he was free from all his complaints; his digestion was quite restored, and intelligence was brought him at the end of a week that his patient continued in good health.

It is rather a singular circumstance that these are the only two cases of Homöopathical cures reported by Hahnemann, although many are scattered up and down in the writings of his disciples. They are sufficient to show that the system utterly fails when brought to the test of experience, and however ingenious the theory may sound, it appears too ridiculous in its application ever to obtain supporters on this side of the channel. Although the Homöopathic party is still numerous in Germany, it is but right to say that its popularity appears to be deservedly on the wane, and it is probable that it will die a natural death in the days of the next generation.

5, New North Street, April 14, 1827.

ART. VII.—*On the Detection of Antimony in Mixed Fluids.*

By EDWARD TURNER, M. D. F. R. S. E. Fellow of the Royal College of Physicians, and Lecturer on Chemistry, Edinburgh.

HAVING been recently engaged, along with Dr Christison, in examining some food supposed to contain tartar emetic, I was led to inquire into the comparative value of the tests recommended for detecting that substance; and as, on pursuing the investigation, I found reason to distrust the method described in our best works on toxicology, and at the same time succeeded in rendering it more secure, I am induced to believe that a short account of my experiments will not be unacceptable to the public.

Many reagents decompose tartar emetic, and cause precipitates in its solution. Of these the principal are alkaline substances, the stronger acids, such as the muriatic and sulphuric, the infusion of gall-nuts, and sulphuretted hydrogen. The

value of these tests is very unequal. Pure potash, when cautiously added to a strong solution of tartar emetic, occasions a pretty copious flocculent white precipitate, which is readily and completely redissolved by an excess of the alkali. In a moderately dilute solution potash does not produce any change. Pure ammonia in a concentrated solution throws down a white, very fine, granular precipitate, which adheres firmly to the glass, and is only partially redissolved by an excess of the precipitant. Tartar emetic is not precipitated by carbonate of ammonia. The fixed alkaline carbonates and lime water act with considerable delicacy. In a solution containing a grain of tartar emetic to an ounce of distilled water, carbonate of potash and lime water yield distinct white precipitates, that from the former being the protoxide of antimony with a little carbonic acid, and that from the latter consisting of the tartrates of antimony and lime; whereas in the same liquid pure potash produces no change, and ammonia a cloudiness scarcely visible. When the tartar emetic is in the proportion of one grain to two ounces of water, lime-water has no effect, but the carbonate of potash still gives rise to a precipitate. If the proportion is a grain to four ounces of water, the action of the alkali can no longer be traced.

The delicacy of muriatic or sulphuric acid as a test of tartar emetic, is almost exactly the same as that of the carbonate of potash; but the acid must be added cautiously, as an excess of it redissolves the precipitate.

The recent infusion of gall-nuts produces a copious yellowish white precipitate in a concentrated solution of tartar emetic. The liquid is rendered turbid, when the proportion is two grains to an ounce; but it undergoes no change when the tartar emetic is in the ratio of one grain to an ounce of water.

Sulphuretted hydrogen acts with far greater delicacy and certainty than any of the others. On transmitting this gas through eight ounces of water containing one grain of tartar emetic, the solution instantly acquired an orange colour; and after saturating the liquid with the gas, and boiling in order to expel the excess of it, a considerable quantity of the sulphuret of antimony quickly separated.*

From these experiments it fully appears, that of all the tests of tartar emetic enumerated by toxicologists, sulphuretted hydrogen is the only one which is sufficiently delicate for being entitled to confidence. It is the only one, also, the indications of which as to the presence of antimony are precise. The

* This precipitate is commonly, but I conceive incorrectly, regarded as a hydrosulphuret of the oxide of antimony. It appears rather to be a hydrated sulphuret of the metal.

orange tint of the precipitated sulphuret of antimony can scarcely be mistaken for any other metallic sulphuret by a person acquainted with its appearance. Its colour is quite different from that of orpiment or of the bisulphuret of tin; and from the sulphuret of cadmium, to which it bears a greater resemblance, it is distinguished by its ready solubility in a solution of pure potash. On the contrary, the other tests, taken singly, supply no proof whatever of the presence of tartar emetic; though, when they all agree in their indications, their evidence is not likely to be deceptive.

In describing sulphuretted hydrogen as a test of tartar emetic, it is almost unnecessary to state that this gas merely indicates the presence of antimony, without directly showing in what state it existed. But since tartar emetic is the only pharmaceutical preparation of antimony which is soluble in water, the detection of the metal itself, in judicial cases, leaves little doubt of its having been in the form of the double tartrate. This, however, is not a point of much importance, because all soluble antimonials are poisonous.

In order to ascertain if sulphuretted hydrogen may be relied on for discovering the presence of antimony in complex animal and vegetable fluids, tartar emetic, dissolved in water, was mixed with tea, broth, porter, and milk, in such quantity that each solution amounted to four ounces, and contained two grains of the compound. Through these solutions, after being acidulated with tartaric acid, boiled and filtered, a current of sulphuretted hydrogen gas was transmitted during fifteen or twenty minutes. In the three first liquids an abundant precipitation ensued immediately, and the same took place in the milk after boiling. The precipitate subsided easily from each, and the colour of that from the tea, broth, and milk, was quite characteristic. That procured from the porter was not so satisfactory at first; but on collecting and drying it upon a filter, the paper presented the distinct orange tint of the precipitated sulphuret of antimony.

In recommending the use of tartaric acid, I may observe that the employment of this substance should in no case be omitted. According to my observation, all the precipitates occasioned in tartar emetic by reagents, sulphuretted hydrogen excepted, as well as by animal or vegetable fluids, are readily dissolved by tartaric acid. Thus the precipitates occasioned by lime-water, or muriatic acid, disappear instantly on the addition of tartaric acid; and the compound of tannin and the oxide of antimony, whether formed by the infusion of gall-nuts, tea, or cinchona bark, may easily be rendered soluble by the same means. If milk is present, muriatic acid should likewise

be employed, by which the coagulation of the caseous matter is more completely effected. In order, therefore, to form a rule applicable to every case, the following directions may be given : The fluid supposed to contain tartar emetic should be mixed with a drachm or two of muriatic and tartaric acids, boiled for a few minutes to separate any substance coagulable by heat, and then allowed to cool, and filtered. The liquid should next be exposed to the action of sulphuretted hydrogen, and boiled to expel the excess of the gas ; after which the sulphuret will subside if tartar emetic had been present.

After procuring the sulphuret of antimony by the process above described, it is important to subject the compound to some operation by which the metal may be obtained in a separate state. Professor Orfila, in his work on Toxicology, (Vol. i. p. 465, third edition,) states, that the precipitate in question, “ dried on a filter, and mixed with charcoal and the potash of commerce, gives a button of metallic antimony by the action of heat. This reduction of the oxide of antimony by charcoal may be made in an earthen crucible, and is completed in the space of about ten or twelve minutes.” It is chiefly to this part of the process for detecting antimony that I have found reason to object. I do not, indeed, deny that the process will succeed perfectly, when a considerable quantity of the materials is employed ; but in operating on such quantities as are likely to be met with in medico-legal investigations, my attempts to procure the metal in this way have proved completely fruitless. Thus, four grains of the sulphuret, precipitated from tartaremetic by sulphuretted hydrogen, and well dried, were mixed with an equal weight of charcoal and dry carbonate of potash. The mixture, protected on all sides by charcoal, was placed in a Hessian crucible, carefully luted, and was then exposed to heat during fifteen minutes. The experiment was twice repeated ; and on one occasion a full red, and on the other a commencing white heat was employed, but in neither case could I perceive any trace of the metal. On examining the residue chemically, I found that some particles of metallic antimony were diffused through the mass, though they could not be discovered by the eye ; while another portion still remained as sulphuret, and was dissolved by the potash on the addition of water. These experiments were varied by mixing the sulphuret with black flux, and heating the mixture in a glass tube by means of a spirit lamp ; but a metallic globule was not procured, though the heat was augmented by aid of the blowpipe. It is worthy of remark, that in none of these trials was there any appearance of a metallic sublimate ; so that, were colour insufficient for dis-

tinguishing orpiment from the sulphuret of antimony, the black flux would afford an easy mode of distinction.

Having failed in my attempts to procure the metal by the preceding process, I had recourse to another which proved successful. It is founded on the property hydrogen is known to possess, of separating sulphur from antimony at an elevated temperature,—a property, of which advantage has been taken for the purposes of analysis. In performing this operation, the dry sulphuret is placed in the middle of a glass tube about three inches long, and a quarter of an inch in diameter. One end of the tube is connected by means of a cork with a vessel, from which hydrogen gas is evolved; and to its other extremity is adapted a bent tube, which opens under water, so as to conduct away the hydrogen, and at the same time exclude atmospheric air. After the air within the apparatus has been expelled, heat is applied by means of a spirit lamp to the part of the tube on which the sulphuret is placed. The decomposition of the sulphuret commences at a temperature by no means elevated; but in order to render it complete and fuse the antimony, the glass should be made red hot, and kept in that state for five or six minutes. The temperature at the close of the process may with advantage be increased to bright redness by the use of the blowpipe.

The appearance of the metal within the tube depends upon the manner of conducting the experiment. If the sulphuret had been placed in a heap, the metal is found partly in a spongy state, and partly in minute globules; but if it had been diffused over a considerable space, no globules appear, and the metallic lustre is indistinct. The metallic nature of the spongy mass may, in general, be brought distinctly into view by placing it on a piece of white paper, and pressing it with the nail or the blade of a penknife.

The result also depends on the velocity with which the hydrogen is transmitted through the tube. If the gas passes rapidly, some of the metal is hurried off at the moment of separation from the sulphur, and is deposited within the tube as a metallic film, which is sometimes very distinct. If, on the contrary, the passage of the gas is slow, this appearance does not take place.

By means of this process, I have succeeded in procuring from the tenth of a grain of the sulphuret metallic antimony, the lustre of which could be distinctly seen with the assistance of a lens. From half of the precipitates procured from the mixture of two grains of tartar emetic with broth and milk, I procured distinct metallic globules.

Should a considerable quantity of animal or vegetable matter

subside with the sulphuret, the metallic antimony will then be so mixed with charcoal that its lustre cannot be seen distinctly. This occurred to me in decomposing the sulphuret obtained from porter. In a case of this kind the mixture should be placed in an open tube, and heated to redness by means of a spirit lamp. The antimony is then oxidized, and the oxide, which attaches itself to the cool parts of the tube in form of a white powder, may be recognized by its appearance and volatility.

ART. VIII.—*A Case of Inflammation of the Tongue cured by Incisions.* By CHARLES MARTIN, Surgeon, Island of Mull. Communicated by Dr JOHN AITKEN.

A STOUT healthy man, aged 35, came on the ninth of February last, to consult me about the state of his tongue. On interrogating him he gave me the following particulars.—

On the 7th he was employed in building the wall of a house. Not long after he returned home in the evening from his work he felt an acute pain in the tongue, at first so circumscribed that it could be covered with the point of the forefinger, but speedily extending over the whole surface of the organ. The pain was attended with a most distressing sensation of heat, and a feeling as if the tongue was gradually and uniformly enlarging. The lingual arteries throbbed violently, and the saliva adhered tenaciously to the roof of the mouth and surface of the tongue. He was restless and a little feverish, and complained of headach. With little variation in the symptoms, the tongue continued to increase to such a size that it protruded from the mouth, separating the jaws to the extent at least of half an inch. When the disease assumed this aspect, he became dreadfully alarmed, and between twelve and two o'clock called upon me.

On examining the tongue I found that it occupied a large proportion of the cavity of the mouth; and I could with difficulty introduce my little finger between it and the upper jaw. It felt smooth and hard to the touch, and had a thick coating of viscid mucus. From the high degree of tension its point presented a glistening appearance. The patient had his jaws wrapped up in flannel; his countenance betrayed great anxiety; his pulse beat strong and frequent, and his skin was hot.

The treatment adopted was as follows:—

I opened a vein at the bend of the arm, and withdrew blood

to the extent of thirty ounces. Having secured the wound, I remained a few minutes to observe the result of the bleeding; and before I quitted the apartment he thought himself rather relieved. In an hour afterwards I returned and repeated the venesection to the same extent, which produced good effects, for he could articulate somewhat intelligibly. I had hopes that the swelling would gradually decrease without farther interference with the lancet; but if not, I was decided upon the steps I should next pursue.

Three hours afterwards I repeated my visit; but instead of finding, as I expected, evident marks of improvement, the disease, on the contrary, had rapidly increased. At this period respiration through the mouth was totally suspended; and he could not breathe even through the nostrils but with difficulty. His countenance was flushed and anxious; the pulse was fluttering; his breath offensive. In short, he was threatened with immediate suffocation.

It was now perfectly obvious that the only remedy by which the patient's life could be saved was the knife. His head being properly secured, and the mouth opened as wide as possible, a scalpel was introduced, with its flat surface along the tongue, and a deep incision was made in the most prominent part of the right side. On withdrawing the scalpel a quantity of blood, mixed with *puriform* matter, issued from the wound. In a very short time he felt relieved. When this wound ceased to discharge blood, I made other two incisions, one in the middle, and the other in the left side of the tongue, but no purulent matter followed. The bleeding was promoted by gargling the mouth with warm water.

This local treatment produced such a happy and immediate effect, that, in a quarter of an hour after the first incision was made, the patient could articulate words pretty distinctly; his respiration became free and easy, and his countenance lost its anxiety. The joy he felt at this sudden and favourable change was extreme. He was, to use his own terms, like a man unexpectedly snatched from the grave.

On the following morning I paid him a visit. I found him sitting on the edge of the bed quite cheerful and happy. I left him with strict injunctions that he should avoid exposure to cold, and gave him a few purgative medicines. A few hours thereafter he was walking in the street with his head fortified with flannel.

Remarks.—In conclusion, it may not be altogether nugatory to offer a very few observations upon the preceding case.

1. That the disease was inflammation, and that violent

in its symptoms, and consequently rapid in its progress, is evident from the fact that purulent matter was largely secreted. Previous to the first incision being made, I could not possibly determine, either from existing symptoms, or by external examination, whether or not there was a collection of pus. Could I have satisfied myself of this, I would not have prolonged the patient's sufferings by employing the lancet, but would have had immediate recourse to the knife. As the matter was deeply lodged in the substance of the tongue, I remained ignorant of its formation until the scalpel was used.

2. It is hardly necessary to remind the reader, that, to effect any real and permanent good, the incisions should be made deep and extensive, otherwise he is only torturing his patient with pain—likely to prove more injurious than beneficial. The operation is exceedingly simple, and is not attended with the smallest danger, so that the surgeon may safely act with boldness and promptitude. Indeed, such is the urgency of the case that it imperatively demands vigorous interference; and trifling remedies are only dangerous instruments in the hands of a surgeon. In the case related above, it is questionable if the man could have survived for any length of time, unless the enlargement had been subdued by the timely use of the knife; for the pus was so deeply seated that he might have been suffocated before it could make its way outwards.

3. It may be observed, that, if this disease is early combated by copious depletion with the lancet, it may, as in other cases of inflammation, be speedily cured; but when this treatment proves ineffectual, I know of no other remedy by which the patient's life may be saved except incisions.

4. And lastly, I have been induced to lay the preceding case before the public, because it is one, among many others, which sanctions the propriety, and illustrates the efficacy of having recourse to the scalpel when general blood-letting fails to produce the desired effect. *

*7th April,
Mull, Argyllshire.*

* For a similar case to the foregoing, see this Journal, No. xxi. p. 235.

ART. IX.—*Case of Cynanche Laryngea, in which the operation of Bronchotomy was successfully performed.* By WILLIAM CULLEN, M.D. F.R.C. Surg. Lecturer on Anatomy, and one of the Surgeons of the Royal Infirmary, Edinburgh, &c. &c.

(Read at the Medico-Chirurgical Society of Edinburgh.)

ON the morning of the 14th of March, about five o'clock, I was requested to see the subject of the following observation, Mary Bone, aged 35, residing on the Castle-Hill. I found her labouring under a very severe affection of the breathing, attended with long, and difficult, and rare inspiration, during which the muscles of the neck and the *alæ nasi* were called into action, and the blood was evidently sucked forcibly out of the jugular veins; expiration was short, but otherwise easy; the face was very much flushed, and bathed in perspiration; the eyes prominent and red; and the whole countenance expressive of the utmost anxiety. She had occasional paroxysms of coughing *without expectoration*; the pulse was at 120, full and soft; the skin moist; the voice uttered in whispers only. She pointed to the larynx as the seat of severe pain, and mentioned that she felt as if a heavy weight had been laid upon her breast-bone. On percussion the whole of the chest sounded well; by the stethoscope, respiration was very faintly heard, but without any rôle.

The patient reported that she had had a bad cold since the month of November, with cough, difficult breathing, and expectoration; the voice, she said, she had gradually lost; she could speak very little in cold or damp weather, but more easily on fine, clear, warm days. All the symptoms were aggravated during the night. She had not been under proper medical treatment. A few days before my visit she partook plentifully of a mixture of black beer and whisky, with a view to promote sweating, but this evacuation did not take place, and her symptoms were much aggravated by the dose.

On comparing the symptoms with the history of the case, I was quite convinced that the patient laboured under *Cynanche Laryngea* in the chronic form, with great obstruction in the *rima glottidis* threatening suffocation. The texture of the lungs, and the minute branches of the bronchi, I imagined to be sound, or nearly so, in consequence of the sonorousness of the chest, and perfectly natural sound of the breathing on listening with the stethoscope.

As the state of the symptoms did not indicate bleeding, and the history of the patient and her strength did not seem to admit of it, I contented myself with giving her a brisk purgative of

compound powder of jalap, followed at the end of an hour by a draught of opium and ether. A large blister was applied to the sternum, and the feet were bathed in warm water.

At half-past ten A. M. there was no improvement in the symptoms; the antispasmodic draught had given slight, but only temporary, relief; and the woman, now for the first time, gave me reason to fear spasm of the glottis, by informing me, that within the last fortnight she was occasionally quite easy, with natural respiration, while sometimes her breathing became suddenly exceedingly laborious, and continued so for half an hour or an hour at a time.

At 1 P. M. the purgative had operated well; the blister was beginning to rise. Not only, however, was there no amendment, but the symptoms were even worse than before. The breathing, inspiration chiefly, was very slow and laborious, attended with alternate rising and emptying of the jugular veins; the cough was frequent, still without expectoration, and she now complained of acute pain of the right side, increased on pressure between the intercostal spaces, or by full inspiration. The pulse was 130, small and weak; the countenance flushed; skin hot and dry; look anxious; indeed she seemed on the point of suffocation. Believing her in the utmost and immediate danger, and that there was no time to wait the result of further medical treatment; and deeming bronchotomy to be the only chance of safety left, with the approbation and assistance of my friend Dr Craigie, I proceeded immediately to the operation.

An incision, two inches and a half in length, beginning about the middle of the thyroid cartilage, and terminating over the isthmus of the thyroid gland, was made exactly on the median line of the neck through the skin and superficial fascia. Over the ligament that unites the cricoid and thyroid cartilages anteriorly, I readily found the cellular interspace which separates the muscles of the right from those on the left side; between which I dissected down to the crico-thyroid membrane. The latter was cut longitudinally; but, as the incision was too small to admit the large curved tube generally employed by Mr Liston and others, I enlarged the wound to a proper size by slitting up the lower third of the thyroid cartilage, and then succeeded in introducing the tube. In the operation an artery of considerable size was divided; it was not large enough, however, to admit of a ligature. As soon as the larynx was opened, at each inspiration blood from this vessel was *sucked in by the wound*; but luckily it did not annoy the patient, and I was not forced to employ any artificial means to stop it. On the first introduction of the tube a fit of coughing supervened, attended, to my great surprise, with expectoration through the

canula, of a tough semitransparent mucus, slightly tinged with blood. The expectoration was not free, and the matter could hardly be expelled, so that it was necessary to extract it from the tube by forceps, and other similar means. The cough soon subsided in a great measure, and then we had an opportunity of observing that the respiration was much easier than before; and the woman said she felt very much relieved.

10 P. M.—The tube has remained quite steady in its situation, undisturbed by the motion or coughing of the patient. Her state is highly satisfactory; respiration is quite free; cough not frequent, and attended with expectoration as before; pulse 110, full but soft; countenance of good expression, not flushed; heat of surface moderate; complains chiefly of want of sleep. An anodyne draught, with forty drops of the sedative solution of opium, was advised to be taken immediately; and a drachm of compound powder of jalap was directed to be given in the morning.

15th, 10 A. M.—Has passed a good night, with tranquil and refreshing sleep; cough, with expectoration, continues, but the breathing is free; pulse 100, of good strength; powder has operated well; a pectoral mixture, with syrup of squill, to be taken occasionally.

8 P. M.—Cough rather more troublesome; other symptoms as before; pulse rather feeble, with disposition to sweat; ordered to have occasionally a tea spoonful of wine and water; the anodyne draught and purgative to be taken as before.

16th.—Another good night; respiration continues easy. She has been troubled this morning with sickness and occasional vomiting; and complains of pain of right side, increased on full inspiration or pressure. The wine to be stopped—the cough mixture to be continued.

9 P. M.—Nausea and vomiting have entirely ceased, but the pain of side is aggravated. The right side, at its lower part, sounds rather dull on percussion, and the breathing can hardly be heard with the stethoscope; cough occasionally severe; expectoration very viscid, and can with difficulty be removed from the tube; pulse 100, full; heat of surface considerable. To have a dozen leeches to the side, and a poultice applied; anodyne draught and purgative to be repeated; squill mixture to be stopped.

17th.—Leeches bled well, and pain of side greatly relieved; respiration free; cough moderate.

For the next week the symptoms continued to improve. The respiration gradually became more free, the cough less frequent, and the expectoration more and more transparent, and so viscid, that the tube sometimes became nearly obstructed by

it. The pain of the side gradually abated; the sonorousness of the chest returned; respiration was again heard; and the pulse sunk to its natural standard. A moderate quantity of food was allowed; the patient gradually regained her strength, and described herself as free of complaint of any kind. The presence of the tube gave no annoyance or pain. The edges of the incision granulated, and yielded a natural, purulent, and moderate discharge.

With regard to the voice, I may remark, that, after the operation, she could not make herself understood as long as the tube was left open; when she placed her finger on the mouth of it she was then able to speak distinctly, but in whispers only.

The patient remained in the state above described till about the 28th of the month, a full fortnight after the operation, when the voice having become stronger, and no cough or difficulty of breathing remaining, I withdrew the tube. The wound kept open, and the patient breathed through it with great ease and tranquillity. In a day or two afterwards I brought the edges together, and covered the whole with adhesive strap and bandage, so that the patient now had the use of the larynx restored. This stoppage of the wound, and restoration of the natural passage, were very well borne, and it then became my object to heal the opening as quickly as possible by the application of suitable dressings. In a short time the whole wound was filled with granulations, but no contraction of them took place, so that there existed during a certain period a line or fissure as it were, through which the probe could easily be introduced into the larynx. This fissure, however, did not serve for the introduction of air from without; and it was only during a fit of coughing, or in a hurried attempt to speak, that a small portion of air escaped from the trachea with a distinct crepitus, indicative of the difficulty it had to pass.

By the application of the sulphate of copper twice or thrice the contraction of the granulations was completed, and the wound was perfectly healed on the 20th of April, about seven weeks from the date of the operation.

On reflecting on the history of the above case, I am quite satisfied that the operation was called for by the state of the disease, and that I had a right to expect a successful issue from the affection being confined to the larynx. I was inclined to think the absence of expectoration a very favourable symptom for the following reason. In several of the cases in which I have seen bronchotomy performed, there was previously a copious expectoration, which immediately after became difficult, and sometimes entirely suppressed, in spite of great efforts to cough on the part of the patient. But, indeed, as soon as

an opening is made into the trachea, the patient can no longer cough, and, of course, he can hardly expectorate. Coughing is a compound action. The first part of it consists of a full inspiration, followed by the strong action of the constrictor muscles of the glottis, which the expiratory muscles attempt to force. The second part consists of the sudden relaxation of the muscles of the glottis coinciding with a violent expiration, during which, and by means of which, mucus, pus, or other matters in the windpipe and *bronchi* are brought up by coughing. But as soon as an opening has been made below the *rima glottidis*, the action of the constrictor muscles cannot take place; and, therefore, the expiratory muscles cannot acquire sufficient force by acting against their antagonists, so that, in such circumstances, the patient cannot cough, he can only expire, and produce a current of air too weak for the purposes of expectoration. Hence it is, that expectoration becomes sometimes so difficult after bronchotomy; hence it is that the entrance of a small quantity of blood is sometimes fatal. The patient cannot get rid of the mucus or blood, because he cannot cough; he can only expire. I remember to have seen bronchotomy performed on a stout and vigorous young man, threatened with immediate suffocation from an affection of the larynx. Previous to the operation he could expectorate, though with some difficulty; but still he brought up a sufficient quantity. As soon as the opening was made, the expectoration became much more laborious; the sputa were brought up by expiration, and that with so little force as to be hardly driven through the canula. In a few minutes he died suffocated; and after death we found, filling the trachea for about an inch, a small quantity of coagulated blood, which, in its fluid state, should have been easily expelled had the patient been able to cough.

Hence it is that I consider the absence of expectoration a favourable symptom, both because there will be no risk of suppressing its expulsion by the opening, and because it indicates the absence of inflammation of the bronchi. In the case of Mrs Bone, expectoration occurred immediately after the operation; which, I conceive, depended on the glottis being so far contracted, that a quantity of air sufficient for a forcible expiration could not be taken in; but when the canula was introduced, then the inspiratory part of the process became complete, and the expiratory difficult on account of the want of the glottis, the invariable effect of the new opening below.

On the whole, then, I have reason to be satisfied with the issue of this case, and recommend it to the notice of the society, as illustrating some points of importance in deciding the question as to operating.

ART. X.—*Reply to Professor CHRISTISON's Criticism in the Edinburgh Medical and Surgical Journal for April 1827, on Dr MACKINTOSH's Evidence in the case of Mrs SMITH, tried for Poisoning before the High Court of Justiciary.**

To the Editors of the Edinburgh Medical and Surgical Journal.

GENTLEMEN,

IN the last Number of your Journal, Professor Christison published an account of the medical evidence which was elicited during the late trial of Mrs Smith for poisoning, in which he has thought proper to criticise my evidence. I therefore trust you will have the goodness to insert the following reply in your next Number.

It was stated by me, that, "*If the mixture owed its whiteness and consistence to arsenic, it must have been a dreadful dose; and it is scarcely possible but that violent symptoms must have followed speedily.*"

Upon this Professor Christison has been pleased to make the following remark: "Dr Mackintosh seems *not to have known* that persons have died without pain, vomiting, and purging; but this absence of the usual symptoms occurs only when death ensues in a few hours." Query. Does Professor Christison not consider *death* a violent symptom? From the tenor of the whole paper it would appear that the Professor holds me very cheap indeed; but surely every medical man is acquainted with the fact, that sudden death may be produced by poisons taken into the stomach, and acting at once on the nervous system. I cannot see any difference between my statement and his remark. Surely, the Professor meant only to show a little subtilty, by quibbling on the word "violent," as if it were applied on that occasion only to signify "pain, vomiting, and purging."

My statement continues. "*Norrie, if she partook of such a mixture, would have felt a very acrid taste if it had been arsenic.*"

In Note 32 Professor Christison says, "see my evidence." On reference to his evidence it will be found to run thus, "Different accounts have been given of the taste of arsenic. There have been instances where the taste was acrid: In others it has been described as sweetish; and I know one instance where it had no taste at all. *At a judicial examination a white powder was delivered to me for analysis; a surgeon present asked to see it, and tasting it, pro-*

* On account of the importance of the topics embraced by this and the succeeding paper, and other circumstances which it is unnecessary to mention, we have departed from our usual practice of declining to admit articles of the nature of personal controversy. We have done so, however, on the understanding that the controversy should be concluded in the present number. Dr Mackintosh has been offered the right of returning a farther answer to Dr Christison's reply; but he has adhered to his intention announced at the end of this article,—and of course has not seen Dr Christison's observations previous to their being published.

nounced it to be, as the people supposed it, magnesia, for he felt no taste. I found, nevertheless, on analyzing it, that it was pure arsenic."

It may be the duty of a Professor in his lectures, to mention all the exceptions to every general rule, respecting the science which he teaches; but when a medical man gives evidence in a court of justice, more especially when the life of a fellow-creature is at stake, he surely ought to ground his opinions on ordinary events, and not on one or two extraordinary facts, or rather, I ought to call them insulated statements. The members of the profession, I am sure, will go along with me in opinion, that in general the oxide of arsenic communicates an acrid taste to a healthy palate, although the Professor tells us "that it has often been tasted and swallowed too without any taste being perceived at all."

Does the Professor then suppose that all the authors on chemistry, from one or two such facts, will henceforth alter the accounts they have given of the properties of this poison? Thomson says it has a "sharp acrid taste"—and Henry describes it thus, "it has an acrid taste,"—"The solution has an acrid taste." Orfila, a great authority, describes the taste as "acrid and corrosive."

The next point is one of considerable importance, and my evidence was as follows:

"The sickness she (the deceased) felt on Wednesday morning is a very common symptom of pregnancy in the third month; I have frequently seen such symptoms far more severe in pregnant females at that stage."

Professor Christison proposes the following query in Note 33. "Does the acute burning pain also occur in such cases?" You will be surprised to hear that there was no evidence led to show that the deceased had any "acute burning pain" on Wednesday morning, or even during the rest of that day; nor did it appear on the trial that any one saw her vomit on Wednesday. Jean Norrie, her fellow-servant and bed-fellow, stated in her evidence, that, when she herself awoke in the morning of Wednesday, she "saw Warden sitting striking a light to kindle the fire—Warden grew sick—helped her back to bed—could not positively say if she vomited at that time or not,"—but on her cross-examination by Mr Jeffrey she distinctly said she "did not see her vomit on Wednesday."

Another witness, Barbara Baxter Small, stated that she "saw her on the Wednesday and Thursday in bed. On Thursday said her inside was burning, and complained of thirst—witness gave her water—saw her vomiting—she threw up the water immediately after taking it."

Cross-examined.—"Did not see her vomit on Wednesday—saw her frequently that day. Gave her her dinner—a flour cake and about a mutchkin (English pint) of milk. The flour cake was about the ordinary size—about the bigness of a broth plate. She took all that."

Another female witness, Mary Gibson, or Anderson, states that

she saw the deceased in bed on Wednesday ; " she said she had a cold, and had got a sore throat, but was better."

Cross-examined.—" Did not see her vomit at twelve (on Wednesday,) nor hear her vomiting in the evening. Did not hear her complain of *burning*."

Now then, it appears from the evidence of the case, that there was even no vomiting on the Wednesday morning, but merely *sickness*—certainly no "*acute burning pain*" all Wednesday,—therefore I might well say that I had " frequently seen symptoms far more severe in pregnant females at that stage."

But, supposing the deceased had complained, from the very beginning, of "*acute burning pain*," upon which the Professor lays so much stress, I cannot help expressing my surprise that Professor Christison should be unacquainted with one of the most common attendants on pregnancy—HEARTBURN. We *occasionally* meet with instances in which it produces an agony, which only pregnant women can describe. They often represent the pain as if produced by the action of boiling water on the stomach, or of hot lead. The desire in such severe cases for the coldest drink is incessant, which is no sooner swallowed than vomited. There is acute tenderness, increased on pressing the pit of the stomach. There are even sometimes spasms in the abdomen, and cramps in the lower extremities. During the last twelve months I have been consulted in several distressing cases of this kind ; and since the publication of Professor Christison's paper, I have attended a violent case with Dr H. Davidson, and in which premature labour was apparently the consequence of intense suffering from the "*acute burning pain*" in the stomach.

I have been particular upon this subject, because it appears to form an important topic of medical jurisprudence. And, whatever respect I may entertain for Professor Christison's talents, scientific acquirements, and tact in performing chemical experiments, I cannot help stating my conviction, that practical experience acquired at the bedside is of far greater consequence in a case of this nature, in directing judges and juries, than that kind of knowledge which is obtained by reading, even when united with great dexterity in conducting chemical analysis. There never was a case which afforded better proof of this position. The medical gentlemen of Dundee, whose conduct on this occasion is above all praise, detected arsenic just as decidedly as Professor Christison, although they stated in court that they had never performed the like experiments before. I cannot help adding, that Professor Christison appears to me to have presumed too much upon his office, and seems to forget what is due to practical men.

The next point of my evidence which has drawn forth observations from Professor Christison's pen, relates to cholera morbus. I stated, that "*cholera is very common in this country after hot summers. The duration in fatal cases of cholera is various ; it is not an uncommon thing for cholera to prove fatal even in this country in two or three days. I have myself seen a person die of it in twelve*

hours in this country ; I have seen it both here and in foreign countries."

Professor Christison observes, Note 34, " This is a valuable fact. The question has been put upon trials, (Trial of Mr Donnall in 1817.—See Smith on Medical Evidence, p. 212 ;) but no witness has hitherto been able to say that he knew of an actual case fatal in this country within two days. I may add, that Dr Duncan Senior informs me a gentleman of his acquaintance was attacked by cholera in the Edinburgh Theatre, and died next forenoon. I have also heard of a case that proved fatal in seven hours ; but the patient was not seen by a medical man. Drs Abercrombie, Duncan Junior, Home, Alison, and many others of my acquaintance, never saw a case fatal in *two days*,—so that Dr Mackintosh's opinion as to the commonness of such an event appears *more than problematical*. The terms common, very common, uncommon, are often vaguely used by medical witnesses."

These observations convey a direct and severe censure upon my statement. But I complain most of the twist which Professor Christison has given to more than one part of my evidence ; not so much because it will tend to injure me, but because it may influence the minds of others in similar cases hereafter. I have copied the account of my own evidence from Professor Christison's paper, preserving even his punctuation. "*The duration in fatal cases of cholera is various ; it is not an uncommon thing for cholera to prove fatal even in this country in two or three days.*"

The antecedent to "*uncommon thing*" is "*fatal cases.*"—This was my meaning when delivering my evidence, it is my meaning now ; so that my evidence would read thus: *It is not an uncommon thing, in fatal cases of cholera, for death to happen in two or three days.* It is distinctly within my recollection, that I had in view to counteract the impression which Dr Taylor's evidence was likely to produce on the minds of the court. That gentleman stated, " The cholera in my part of the country never proved fatal sooner than fourteen days." In my cross-examination by the Lord Advocate, I stated " that cholera could not prove fatal as cholera so late as fourteen days ; death is then owing to some other disease lighted up in the system."—" It is common for cholera even in this country to prove fatal in two or three days, *if neglected*. Every practical man knows that cholera in this climate is in general a manageable disorder, if taken in time ; but, if neglected, it is not so manageable. I have myself seen either five or six adults die of cholera neglected ; and they have all died within two or three days. I cannot tell how many children have died of it within my knowledge in less than three days ; and upon inquiry I find that several gentlemen in Edinburgh have met with fatal cases within twenty-four hours.

As Professor Christison did not understand my meaning on this subject, more particularly as his interpretation of it so widely differed from what he had heard or read, surely it would have been more consistent with the courtesy which one medical man owes to

another, to ask for a written explanation ; nor would it have been productive of much trouble, his residence and mine not being above five minutes walk distant from each other. I feel this the more, when I recollect that the Professor did me the honour to call twice, and consulted me upon a very trifling injury, which he imagined had been inflicted on himself, by the public press in this very case.

The Professor, however, it appears, did consult a great number of medical acquaintances upon the subject. Dr Duncan Senior mentioned one fatal case within twenty-four hours ; and he *heard* of another one fatal in seven hours. " But," says he, " Drs Abercrombie, Duncan Junior, Home, Alison, and many others of my acquaintance, never saw a case fatal in *two days*." My statement, both in my examination and cross, was *two or three days, not two days*. This is a trifling mistake for any other person to have made ; but Professor Christison, who has more than once set himself up as the *ne plus ultra* of correctness, an enemy to vague language, and a censor of medical evidence, should have been more careful. I do not know how those gentlemen will relish having their names brought forward upon, perhaps, a casual conversation ; and we all know how vaguely these conversations are conducted.

It is very possible that Dr Abercrombie, in a casual conversation, may have given such a statement, without, at the moment, recollecting every event of the kind which has occurred in his extensive practice ; but a medical gentleman in Edinburgh, whose name I do not think it necessary to mention, unless the fact is disputed, voluntarily mentioned to me, in reference to Dr Christison's statement, that he could have reminded Dr Abercrombie of a case which he saw along with him several years ago, and which was fatal within twenty-four hours.*

I shall not comment upon this ; I only hope Professor Christison will take warning to avoid an error, into which, perhaps, his want of a sufficient knowledge of the world, and etiquette, has led him to fall.

Professor Christison has stated, in the 34th Note, in allusion to cholera not proving quickly fatal, "*The question has been put upon trials, but no witness has hitherto been able to say, that he knew of an actual case fatal in this country within two days*." And, in proof of this, he refers to the " Trial of Mr Donnall in 1817.—See Smith on Medical Evidence, p. 212."

I can only judge by what I have seen with my own eyes, and by what I have read in the records of medicine. On perusing the very trial in the work to which Professor Christison has referred, I find that the first medical evidence, Dr Richard Edwards of Falmouth, gave his testimony in the following manner. Question. " In the course of *your experience* how soon does cholera morbus produce death ?"—" *In general not in less than two or three days ; there may be some instances, but I never met with one that produced*

* I have reason to believe he died in about six hours.

death in less than that time." Mrs Downing, for whose murder Donnell was tried, took the fatal dose about six o'clock one Sunday evening, and died next morning at eight o'clock, being a period of fourteen hours.* Dr Edwards was reminded of this, and then asked the following question: "Can you tell me of any instance that cholera morbus would produce death in so short a time?" Answer. "I never heard, or knew of any instance of its producing death in so short a time;" (of course he means as fourteen hours.) Here then is one evidence stating, upon *his own experience*, that in general that disease does not produce death in less than two or three days; and that he never knew nor heard of any case of cholera proving fatal in *fourteen hours*. This is most extraordinary testimony from the very record to which Professor Christison referred to prove the contrary.

But this is not all, for Dr Adam Neal, a physician at Exeter, another witness on the same trial, upon being asked "what should be the usual course of attack of cholera morbus as to duration, supposing the patient ultimately died of it?" made the following reply: "*It very frequently kills the patient within twenty-four hours*, and if neglected, or improperly treated, it kills the patient in a much shorter period."† These statements are clear and precise, and there is nothing in them which can entitle Professor Christison to say, that "*the question has been put upon trials, but no witness has hitherto been able to say, that he knew of an actual case fatal in this country within two days.*"

In the same case, Dr Daniel, a physician at Exeter, after stating that he had heard the symptoms which Mrs Downing is described to have had the evening before her death, that they are the symptoms of cholera morbus, as well as of arsenic, or any other poison, was asked, "within what period of time does cholera morbus produce death?" replied, "*Within my own experience I have seen it nearly fatal within fourteen hours. I have never known it fatal; I have known a patient in imminent danger within fourteen hours, but he recovered.*"‡

The only remark I shall here make is, that it appears to me to be "*more than problematical*," whether Professor Christison ever read this case in the work which he has so unluckily quoted, to prove the very reverse of what it does establish in the most satisfactory manner.

I have now to take notice of the last objection which Professor Christison has thought it his duty to make to my evidence, respecting the appearances found on dissection; and the profession will agree with me in opinion, that it is one of very mighty consequence in a case of life and death, as a general question; but one which could have had little weight in this case, in leading either the learned judges, or the gentlemen of the jury, to form their opinion

* In Dr Smith's work there is an error of the press at page 213; it represents Mrs Downing's illness to have continued fourteen days, instead of as many hours.

† Smith on Medical Evidence, p. 221.

‡ Smith on Medical Evidence, p. 224.

as to the guilt or innocence of the prisoner. Arsenic was found in the stomach of the deceased; the symptoms were such as might be produced by the action of that poison; so that I had no hesitation in declaring in evidence, that, "*assuming the fact of the detection of arsenic, and taking along with it the symptoms, it is my opinion the deceased died of poisoning with arsenic.*" In this opinion all the medical witnesses concurred.

Professor Christison's report of my evidence runs thus: "Barring the presence of arsenic, I do not consider the morbid appearances described by the Dundee doctors as worth one farthing, because every medical man knows the *post mortem* appearances lose their character in a few days." So far, and so far only, has he reported this part of my evidence with inverted commas, to show, no doubt, that these were the *ipsissima verba* used by me on that occasion. They are not my words. I cannot recollect exactly the question put to me by Mr Jeffrey, or the precise terms of it; but I have reason to recollect my answer, which was as follows: "Barring the presence, (or the evidence,) of arsenic having been found, I do not consider the morbid appearances worth one farthing." I never used the expression, "*Dundee doctors,*" because I should have thought it a term liable to misconstruction. But I believe the learned counsel used that expression in putting the question. I have already expressed my favourable opinion as to the conduct of the "*Dundee doctors*" on this difficult and interesting occasion, and need scarcely say more upon the subject, than that I feel persuaded every medical reader will do well to take their example, as a model to follow on similar occasions.

My evidence is further reported by Professor Christison. "*I have been much accustomed to the examination of dead bodies: I would not take it upon me to pronounce as to the existence of morbid appearances in ten days, much less after three weeks. Even in three days they are (often) doubtful.*"*

Professor Christison seems to have been very much hurt by this statement, as calling in question the accuracy of his powers of observation.

"If he (Dr Mackintosh) did not attach any weight to the pointed terms, in which both reports spoke of the state of the preservation of the alimentary canal, he might at least have given me credit for not being likely to fall into so unpardonable a blunder as the confounding pseudo-morbid with morbid appearances; for he must be aware, that the effects of time and decay on morbid appearances

* In the case of Sir Theodosius Boughton, whose body was not examined till the eleventh day after death, "appearance of inflammation" was described by the medical men to affect "the bowels in the lower, belly," &c. But Mr John Hunter, one of the most celebrated men that ever lived, declared, in his evidence, that "the whole appearances upon the dissection explain *nothing but putrefaction.*"—"I have," (said he,) "dissected some thousands during these thirty years." Paris and Fonblanque, Vol. iii. p. 271.

It is remarkable that the death of Sir T. Boughton also took place at the same time of the year, namely, September.

form an important topic in medical jurisprudence, of which *no professor of that science can be ignorant.*"

If I were to submit to this, I should next expect to be required to pay due deference to the wisdom in the wig or gown which a professor may happen to wear.* I shall say nothing (observes Dr Christison,) of my own experience; neither shall I make observations upon that point, further, than that I should be inclined to pay much more attention to the description of morbid appearances given by the Dundee gentlemen, who are all practical men, in extensive practice, than to that of a young professor, who is not constantly devoted to such pursuits. It would be satisfactory to know how many stomachs Dr Christison has examined three weeks after death, with a view to ascertain the condition of their mucous coat,—and particularly after travelling between sixty and seventy miles in a pill-box by the mail coach?

To come at once to the point at issue: I wish it to be distinctly understood, that it was my opinion at the trial, and it is my opinion at this moment, that the appearances found in the mucous membrane, the product of two or three days inflammation, in any body three and four weeks after death, are not worth one farthing. I am convinced no experienced pathologist is to be found, who will take appearances in the mucous membrane of the stomach and bowels three weeks after death, into the medical evidence of a case, when forming his opinion of the nature and seat of a disease, for his own information and guidance. He will be still more scrupulous when doing so, for the information and guidance of other practitioners; and, above all, he will be more and more guarded when giving evidence before a jury, when the life of a human being is at stake.

I would be willing to leave this part of the question to the decision of the professional public, without further remark, confident that that decision will support the evidence given by me, that no faith ought to be placed on appearances found on the mucous membrane of the stomach and bowels three weeks after death. Had not Professor Christison made an appeal to the facts, I agree with him entirely, that "it is far better that the facts of the question speak for their own accuracy;" because it never entered into my imagination to doubt their accuracy. The best facts I can allude to are contained in the report of the three physicians, who were called upon to see the body disinterred, and to examine into the condition of the internal organs. I thought to be able to do this from Professor Christison's report of the trial, but, instead of the report itself, I find an abstract, which is too well calculated, (although I dare say accidentally,) to lead a reader of his paper astray; therefore, I applied to Mr David Syme, advocate, who is engaged in publishing the criminal trials, and who kindly sent me a proof of this report; therefore, I can vouch for its accuracy.

* Professor Christison must be aware that I also teach some of the branches of medical jurisprudence, and therefore have had my attention directed to this important subject.

The report, which is drawn up and signed by Drs Ramsay, Johnston, and Taylor, after stating, that in their presence the body of a woman, who was reported to have died under suspicious circumstances, was, on Saturday, 30th September 1826, between two and three o'clock in the afternoon, disinterred in the church-yard of the parish of St Murroes, observes,

"We found the face *exceedingly disfigured and irre recognizable from natural dissolution*; the rest of the corpse under *considerable progress to decay*; the scarf-skin here and there separated, or elevated in large black blisters; the skin *greenish*, and the hairs *loose*; the smell less offensive than was expected, *from the examination being conducted in the open air, with a cool breeze from N. west.*" These are the simple facts of the state of the body. Now Professor Christison, instead of detailing them in this manner, merely gives his *opinion* in the form of an abstract, in the following terms, after stating the fact of the ceremony in the church-yard of St Murroes, he observes, "*that there were marks of considerable putrefaction externally.*" The original report goes on to state as follows, with respect to the contents of the abdomen: "On opening the peritoneum a quantity of gas escaped, *pungent and fetid*. The stomach and bowels were in *wonderful preservation*, having made *less advance to putrefaction than the skin, face, and muscles.*" I leave it to be decided by practical men conversant with morbid appearances, and it is only to such judges I appeal, whether they would place reliance upon any change produced by three days of inflammatory action on the mucous membrane of a subject in such a state of decomposition; I am sure they will approve of the testimony given by me, that, "*Barring the presence of arsenic having been found, I do not consider the morbid appearances worth a farthing.*"

When the medical gentlemen of Dundee notice the state of bowels, it will be observed, that they do not say they were in a good state, or in a natural state, but that they were in wonderful preservation, in reference to the decay of the rest of the body. They do not say the bowels were *not* in a state of putrefaction, or progress to decay, but that they had "*made less advance to putrefaction than the skin, face, and muscles.*" Professor Christison, in his own report, notices the condition of the portion of the stomach and intestines which were sent to him from Dundee; he says they were in a state of "*tolerably good preservation.*" But, in his examination in court, he made the following statement, and I beg to remark that I copy it from his own report of the trial, therefore I suppose it must be correct. "I ought to remark, however," (says he,) "that in this case the stomach and intestines, as stated by the *Dundee gentlemen*, and in *my report*, were in a state of *great preservation.*" I cannot permit myself to make a single remark upon this.

Professor Christison wishes to make it appear that arsenic possesses not only antiseptic powers, (contrary to general belief,) but, at the same time, that it preserves appearances of inflammation of three

days standing. "Arsenic" (says he) "has been detected in the body fourteen months after interment;" and maggots attacked and destroyed the bladder which contained the stomach and bowels of poor Warden six weeks after death, "while the stomach and intestines were quite entire, and not a maggot had touched them,"*—this only proves that the maggots were too wise to eat poison; at all events, that *they* could tell *arsenic* from *magnesia* by the taste.

Were it of consequence in illustrating any important feature in this melancholy case, Professor Christison has afforded me ample matter to indulge in criticism both in his own evidence, and in his comments upon the evidence of others,—but I have no taste for such a task; and therefore I shall only farther observe, with respect to the morbid appearances, that mere vascularity in any texture of the body is no proof of the existence of previous inflammation; "that irregular coalescing streaks and patches of a deep red colour, which appear to be produced by blood mixed with the dissolved part of the villous coat, and likewise extravasated and incorporated in its substance,"† may be the product of transudation and decomposition, when discovered in a body three weeks after death; that the villous coat may be "*softened*" as in this case, and a mere touch, performed in the gentlest manner, often produces an appearance of ulceration and abrasion. Air generated by the process of putrefaction between the mucous and subjacent tissues produces partial elevations, which resemble ulcerations and even granulations." Indeed, Professor Christison himself declared, in his evidence, that "arsenic seldom causes death by inflammation; and, in answer to a question put by Lord Meadowbank, the Professor stated, "such ulceration, indeed, is not very common in poisoning with arsenic at all, as death often occurs too soon for the necessary vital process to be accomplished." So that, even granting the appearances in this dissection were to be depended on, if Dr Christison be correct in his statement, they were still not worth one farthing as evidence of the operation of arsenic.

Professor Christison concludes his communication with the following statement: "If I understood correctly the tenor of a conversation I had with Dr Mackintosh *immediately after his evidence was concluded*, the statement which has led to the present comment was intended merely as a general opinion as to the difficulty of distinguishing morbid appearances after a lapse of time. If so, his words bore not that meaning. And if they had, it would have been both an irrelevant and an immaterial meaning, *for I dare say nobody in court needed to be informed, that morbid appearances must fall away with the body into corruption, and that the decay is often rapid.*"

I fear transgressing the bounds of strict prudence were I to give a full reply to this paragraph.

* Other people assert, on the contrary, that there is a putrescent tendency in the bodies of those who die from arsenic. *Vide* Paris and Fonblanque, Vol. ii. p. 229.

† *Vide* Professor Christison's Report.

I remember when I sat down after delivering my evidence, that Professor Christison immediately said something to me. With an ear towards him, and an ear attending to the proceedings, I may have said *something* of my opinion as to the general question, which he construed into such a meaning; but let me ask the profession whether another medical man is justifiable in publishing such conversation, not only *without permission*, but actually without taking the necessary precaution of ascertaining that what he publishes is correct.

In concluding this reply, I have to express my regret for being compelled to engage in a task for which I have neither time nor taste. The tone and nature of the attack made upon my evidence will, I hope, be considered a sufficient justification; and I shall certainly not again encroach on the pages of your Journal, in respect to this matter, however far Professor Christison may be pleased to push it. A feeling of delicacy towards a professor of the same science which I myself teach, has prevented me from saying a great deal more than I have done; and I sincerely trust this lenity may not be lost on Professor Christison's future productions.

I am, Gentlemen, your obedient servant,
JOHN MACKINTOSH, M. D.

Edinburgh, 31, Albany Street,
28th April 1827.

ART. XI.—*Observations on the Duration of Cholera, the taste of Arsenic, and its Power of Preserving from Decay the bodies of those poisoned with it—being an answer to Dr MACKINTOSH's Reply.* By R. CHRISTISON, M. D. Professor of Medical Jurisprudence and Police in the University of Edinburgh, &c. &c.

DR Mackintosh, in one part of his deposition states, in reference to appearances described as morbid by three medical gentlemen and myself; "Barring the evidence of arsenic, I do not consider the morbid appearances described, (viz. in the reports,) *as worth one farthing.*" No one, of course, can find fault with any difference of opinion expressed by Dr Mackintosh, and which he was in duty bound to state in the most explicit terms; but it seems to me that, in a grave judicial proceeding, he ought to have spoken of the depositions of his professional brethren in different language.

In whatever phrase Dr Mackintosh might have expressed himself, I should have still published the substance of my remarks, for the topics under consideration are the fair subject of criticism, and are of great professional and public importance in my own department. According as some of the points are viewed, innocent persons may suffer, or guilty persons escape.

It will not appear strange, that as Dr Mackintosh actually ex-

pressed himself, I did not feel it incumbent on me to qualify any difference of opinion by the general terms of courtesy and compliment which controversialists sometimes employ. Having an example before my eyes however, I cautiously avoided the introduction of any term of an opposite character. Yet simply because I did not address him by way of contrast to himself, in terms of eulogy,—for except the difference of opinion there is no other offence,—he has written the invective now in the *Journal*. I shall pass over every personal allusion in complete silence; first, because I do not wish to obscure some important professional doctrine with the personal concerns of two individuals, about which the public, I at least am sensible, must be profoundly indifferent; and next, because I do not consider myself warranted, on account of certain differences of opinion which have occurred, to deny his general experience, or his practical usefulness in his particular department.

I. The first point relates to the symptoms following a large dose of arsenic. “Where a large dose is taken,” Dr Mackintosh observes, “it is scarcely possible but that violent symptoms must have followed speedily.” The remark upon this is, “Dr Mackintosh seems not to have known that persons have died without pain, vomiting, or purging.”* He now insinuates that his expression conveyed that information to the Court. Is not death, he asks, a violent symptom? I fear that Dr Mackintosh here claims a knowledge of toxicology at the expence of accuracy of expression. I never heard of death as a symptom of disease, or poisoning, and I am sure there was not an individual in Court who interpreted Dr Mackintosh’s deposition as he now does. To illustrate this, suppose the following examination to have occurred, the statement being, that violent symptoms must have speedily followed a dose of poison.

Q. Are you aware that death sometimes occurs without pain, or any violent symptom?

I have already answered that question; an easy death is a violent symptom.

II. The second point relates to the taste of arsenic. “Norrie,” Dr Mackintosh observes, “if she partook of such a mixture, would have felt a very acrid taste, if it had been arsenic,” &c. In my examination, and in the commentary I state, that authors have differed in a singular manner about the taste of arsenic, some saying that it is sweet, others that it is acrid, and two individuals whom I personally knew stating that they perceived no taste at all. I further remarked, that whatever may be its real taste, it has been swallowed without any taste being perceived, because it does not dissolve with ease at the temperature of the body, and so may be swallowed before it impresses the organs of taste. This explanation must be obvious to all, and must be confirmed by their experience, in relation to substances of much more decisive taste than arsenic.

* In fact people have died of arsenic without experiencing any symptom whatever, but slight fainting.

It is plain that my remark on Dr Mackintosh does not apply to the actual taste of arsenic when fully developed, but to his position, that Norrie, if she had taken arsenic, must have felt the taste. I never stated that the taste of arsenic was not acrid, but that there was a difference of opinion upon the point.

Dr Mackintosh quotes Orfila, Thomson, and Henry, systematic writers, and obvious authorities, to prove that the taste is acrid, and sneers at a lecturer, and of course a witness, mentioning rare exceptions. I beg to refer to the following authorities for deciding how far it is safe, without alluding to them, to state, on a criminal trial, that arsenic, if taken, will always have an acrid taste, or even that its taste is really acrid,—and how far Dr Mackintosh is judicious in accusing me of *grounding an opinion on one or two facts, or on insulated statements*. On the trial of Miss Blandy at Oxford in 1763, Dr Addington, speaking of a powder suspected to be arsenic, depones, “This powder is gritty, and *almost insipid* ; so is *white arsenic*.”* At a trial at Aberdeen in 1797, “the surgeon who was called observed a whitish powder among the stuff the deceased threw up ;” and stated, among other reasons for his believing it to have been arsenic, that the deceased informed him “the oatmeal *had a sweetish taste*.”† One of the Mitchells who was poisoned by George Thom, near Montrose, in 1821, felt a *nauseous sweetish taste* in the poisoned porridge. At a very remarkable trial under a false charge of poisoning with arsenic, that of Samuel Whalley at York Spring Assizes in 1821, Mr Walker, a chemist at Leeds, in reference to a taste of copper which the accuser said she felt in some poisoned tarts, depones, “The *taste of arsenic is sweet*, not like copper. If mixed with sweet tart it could not be tasted at all.”‡ Two individuals mentioned by me *did not feel any taste*. Hahnemann, one of the most accurate medico-legal writers on arsenic, says it is sweet. “How easily,” says he, “may children, *on account of its sweetness*, taste and greedily swallow the solution when left for rats and mice.”§ Dr Gordon says, in reference to its supposed acrid taste, “sed non credo experimenta facta hoc satis confirmavisse : nam semper primum dulce, simile saccharo, deinde nonnihil acidum, sed nullo modo tali vi rodendi præditum, ut vulgo existimatur ; saltem in copia quam nobis salus gustare permittit.”|| According to Harles, a very learned and accurate writer on arsenic, the taste of the solid poison is acrid and sweetish, *and develops itself slowly*, that of the solution is *sweetish without acridity*.¶ Hink agrees with Harles, except as to the taste of the solution, which he says is acrid.**

* State Trials, xviii. 1140,

† Burnett on Criminal Law, 549.

‡ Leeds Intelligencer for March 19, 1821. A newspaper, I am aware, is not correct authority ; but the report upon this particular is too pointed and consistent to be distrusted.

§ Ueber die Arsenic-vergiftung, p. 34.

|| Dissertatio Inaug. de Arsenico, Edin. 1814, p. 9.

¶ De usu Arsenici in medicina.

** Ueber Arsenic, p. 36.

Was Dr Mackintosh aware of these authorities, or, knowing them, did he think it safe to state that Norrie would have felt an acrid taste if she had taken arsenic?

It will be observed I do not profess to be certain on the point of taste. It is not proper to be so where authors differ so materially. But in order to contribute my share towards settling a point which, considering the frequency of poisoning with arsenic, appears to me of no small importance, I have made some experiments on the subject, and had others made by several of my friends; and the following is the result. Dr Turner, lecturer on chemistry, tasted two grains, moving the powder over his tongue and palate for half a minute, and perceived no taste. Mr Haidinger, well known as a mineralogist, tried two grains in the same way, and perceived no taste. Dr Becker, an intelligent pupil of this university, also tried it in my presence, and perceived no taste. I have tried two grains often,—once for a whole minute; and towards the close I thought I perceived a very faint sweetish taste, but never any thing else. Dr Duncan Junior tried nearly double the quantity for a minute and a half, and like me thought he perceived a faint sweetish taste towards the end, but nothing more. With regard to the taste of the solution, Dr Turner perceived “an obscure sensation of sweetness;” Mr Haidinger thought he perceived a slight astringency; and to myself the taste was faintly sweet and acid: but none of us remarked any acidity.

What the taste may prove to be, when the poison is allowed to pass to the root of the tongue, I cannot pretend to determine, either personally or by the help of my friends. But the experiments and authorities I have quoted are amply sufficient to determine, that *it may be tasted deliberately without any taste being perceived at all, and that it has been swallowed when mixed with articles of food without causing any sense of acidity.* May not those who say they perceived an acrid taste have confounded with the primary impression the inflammation which is sometimes developed after a considerable interval along with the other symptoms?

III. The third subject of discussion arises out of this statement, “The sickness she, viz. Warden, felt on Wednesday morning is a very common symptom of pregnancy in the third month. I have frequently seen such symptoms far more severe in pregnant females at that stage. The symptoms of cholera, and of poisoning with arsenic, are the same.”* In reference to the sickness of pregnancy I put this query, “Does the acute burning pain also occur in such cases?”

In putting this query I did not mean to dispute any fact, or controvert Dr Mackintosh's opinion, which is not sufficiently explicit to

* This opinion might with justice have been made the subject of remark in my comments. It is correct in regard to the particular case. But as a *general rule*, in which sense it appears to have been Dr Mackintosh's intention to make the statement, it certainly is not correct. There are important exceptions, which, notwithstanding his notions of the duty of a witness, I should think it wrong to exclude.

afford data for discussion. I wished to submit to him and other practical accoucheurs, a query on an important point of which I was ignorant, and upon which the books were silent, Whether the acute burning pain of poisoning is ever in practice observed as a symptom of pregnancy? There was therefore no reason for any irritation on account of this query.

Dr Mackintosh's remarks on the point are twofold. In the first place he says, "You will be surprised to hear that there was no evidence led to show that the deceased had any acute burning pain on Wednesday morning." This statement is not quite exact. The prisoner told Dr Taylor that "Warden had been ill from Tuesday night with vomiting, purging, and pain in the bowels;"* the deceased gave him the same account;† and the prisoner repeated it in her declaration.‡ In my examination, indeed, I have said it is difficult to receive this evidence in opposition to the direct evidence of other witnesses, who had ample opportunities of observing these symptoms on Tuesday and Wednesday, if they did exist. But I cannot conceive why Dr Mackintosh should say there was *no* evidence of the kind.

But secondly, conscious that, although he literally speaks of the sickness on Wednesday, the only relevant comparison was between the affections attendant on pregnancy and the general symptoms of poisoning in the case,—he takes up my query as an abstract question, and replies,—that burning pain, (referring of course to the pain observed in such cases of poisoning as the girl Warden's,) is one of the most common accompaniments of pregnancy,—that he is amazed at my being unacquainted with heartburn,—and that he attended a case in which the burning pain in the stomach was so acute as to cause miscarriage. This is not an answer to my query. When I learn that heartburn, (an affection, by the way, I really have heard of,) is equivalent to a "severe burning pain in the whole bowels," (Report, p. 451,) or, as the girl's mother expressed it, "in all the inside,"—to be more explicit, when I am told that burning pain *in the stomach* is the same thing as the constant burning pain *in the whole bowels, from the stomach down to the anus*, which is a frequent symptom of such aggravated cases of poisoning with arsenic as that of the girl Warden evidently was,—then I shall confess Dr Mackintosh's remark to be pertinent. Meanwhile I confess my ignorance of the existence of this burning pain in pregnancy, and renew the query, sincerely confessing my inferior means of knowledge to Dr Mackintosh, or any practical accoucheur.

V. The next point relates to cholera as a fatal disease. For the information of the unprofessional reader, I may premise the following quotations and remarks: "The progress of *cholera morbus* is also rarely or never fatal in this climate, especially in so short a period as that in which death occurs from the operation of a violent and corrosive poison."—Paris and Fonblanque, ii. 156. So also Dr

* Report, 451.

† 452.

‡ 452.

Smith on Medical Evidence, page 224, in reference to a great deal of rash assertion about the duration of cholera in fatal cases, remarks in a note, "I believe we *must* disclose that in this country *cholera morbus* is seldom fatal." So Dr Edwards, sixteen years a physician, "never had a patient who died of that disease," namely, cholera; and Dr Daniell, for many years a practitioner at Exeter, and physician to the Infirmary there, who "had attended many persons attacked with cholera morbus," states, "I have never known it fatal." (Smith, 218, 224.)

Still farther, two years ago I read before the Medico-Chirurgical Society here, (which, for the information of some, it is proper to explain, is composed of all the leading medical men in Edinburgh,) a paper on some actual cases of poisoning with arsenic, and on some particulars respecting the symptoms. My attention was directed to cholera as the disease whose symptoms are most nearly allied to those of poisoning. One of the most characteristic differences is the more rapid termination of poisoning. I therefore made inquiry at a great many of my acquaintance, including the gentlemen whose names are mentioned in my Report of the trial, whether they ever knew a case of cholera proving fatal in two days and a half. The result of my inquiry I copy from that paper, in preference to stating it now in other words, to show that what I state is not got up for the purpose of controversy. "I doubt whether the cholera of this part of the country ever proves fatal in so short a time as two and a half days; at least, according to the experience of all my acquaintances, it does not." (Edin. Med. Chir. Trans. ii. 327.) This passage I read when there must have been not fewer than forty gentlemen present. After reading the paper, I asked specially whether any one present had seen a case fatal within the period, and no one stated that he had. Some days afterwards I met Dr Abercromby, and, in reference to the reading of that paper, and its future publication, I put to him the same question, and received the same answer.

Now Dr M. observes "cholera is very common in this country after hot summers. The duration in fatal cases of cholera is various; it is not an uncommon thing for cholera to prove fatal even in this country in two or three days. I have myself seen a person die of it in twelve hours in this country," &c. Here follows a statement as to morbid appearances. *Cross-examined.* "Cholera could not prove fatal as cholera in fourteen days. Death is then owing to some other disease lighted up in the system.—It is common for cholera even in this country, to prove fatal in two or three days, if neglected." I am sure no one could ever interpret these passages to mean any thing else than that cholera is not unfrequently a mortal disease in this country, and that it is common for it to run its course in two or three days. I so understood the statement, and felt no occasion, either from its ambiguity, or from the footing on which Dr M. had placed himself with me, to consult him about his meaning.

Now, however, he turns philologist, and it seems, having stated in the outset that the duration of cholera in fatal cases is various, this is

to be what he calls the antecedent to all which follows, so that, when he says "it is not uncommon for cholera to prove fatal in two or three days," this means, that in a disease which, according to all authorities, is very rarely fatal at all, in these rare fatal cases it is common for it to be fatal in two or three days. No comment is required upon this.

Besides, Dr M.'s interpretation of his opinion is inconsistent with his present line of defence; for while, of the various individuals I have mentioned, none have seen more than one fatal case within the period, the disease proving so harmless in their hands, Dr M. states, that, in the course of his experience, within that time, or within three days, five or six adults have perished, and he cannot tell how many children.

Dr M. complains in no measured language of a breach of etiquette in my publishing, "upon PERHAPS a *casual* conversation," the statement of my medical friends regarding the duration of cholera. That is their concern, not his; and does not vindicate his opinion. For my own justification, I appeal to the narrative I have given. Upon a point of fact, carefully investigated, with a view, not to a controversy, but to a general publication, I cannot see the impropriety of giving one's authority. It is said Dr Abercromby at the time had forgot one case. This is natural enough. I find Dr Duncan Junior had actually forgot *one* case. Does this show that it is common for the disease to prove fatal in the period of two days?

Upon the subject of cholera, I have farther remarked in my comments, "The question has been put upon trials; but no witness has hitherto been able to say that he knew of an actual case fatal within two days,"—meaning, of course, that no witness had met with such a case in the course of his personal experience. And I refer to the trial of Donnal in Smith's Medical Evidence. Dr Mackintosh, not content with my simple reference, proceeds to analyse the deposition of the witnesses. He says Dr EDWARDS is asked, "In the course of your experience how soon does cholera morbus produce death?" and answers, "In general not in less than two or three days: there may be some instances, but I never met with one, that produced death in less than that time;"—upon which testimony the Doctor remarks, "Here, then, is one evidence stating, upon *his own experience*, that *in general* that disease does not produce death in less than two or three days." He then adds, that Dr NEALE is asked, "What should be the usual course of an attack of cholera morbus as to duration, supposing the patient ultimately died of it?" and that he answers, "*It very frequently kills the patient within twenty-four hours.*" He next quotes the evidence of Dr DANIELL, who says, "Within *my own experience*, I have seen it *nearly fatal within fourteen hours*—I have never known it fatal; I have known a patient in *imminent danger within fourteen hours*, but he recovered." And he observes, upon all these extracts, that "this is most extraordinary testimony, from the very record to which Professor Christison referred to prove the contrary;" and that

"it appears to him *more than problematical* whether I ever read this case," &c.

Unluckily for his critique, I have read the case with too great care. The evidence of Dr Daniell needs no commentary; I have already put it to its proper use. That of Dr Neale is a general reply to the question, "*What should be the usual course,*" &c.; and it is impossible, even with the help of the context, to interpret his answer as applying only to cholera in this country, or as founded on personal experience, or on any thing else than general knowledge. That knowledge, too, is of the vaguest description; for even Dr Mackintosh must allow that the answer is quite incorrect in regard to the cholera of *this country*—more incorrect than even the Doctor's own reply. As to the answer of Dr Edwards,—it simply declares, in reply to a question regarding the possibility of cholera killing in *fourteen hours*,—that, according to his experience, it does not produce death in *less than* two or three days; and, even standing alone, it cannot be assumed to mean that, according to his experience, the disease actually produces death within that time in this country. This explanation, indeed, if unsupported by other evidence, would infallibly draw down upon me from Dr Mackintosh another charge of quibbling; but it is too well supported by a passage in the case six lines before, which the Doctor has found it convenient to miss out in *his reading*. The same Dr Edwards is asked, "Have you ever had occasion to attend a person who had been ill, and who died of cholera?" And what is his answer? "I NEVER HAD A PATIENT WHO DIED OF THAT DISEASE."

This answer shows the danger of trusting to general questions and answers, of which description are those founded on by Dr Mackintosh, where the line of distinction between general knowledge and *personal* experience is not expressly drawn, and where the witnesses do not in express terms speak of the cholera of this country.

I have only one further remark to make upon the present point. Dr Mackintosh thinks he has been hardly dealt with by my taking the period of *two* days for the course of cholera in remarking on his opinion, instead of two or three days. I have an obvious answer to this complaint. The course of the poisoning was in Warden's case at most three days; probably, according to Dr Mackintosh, as well as myself, less. I, therefore, in regard to the duration of the symptoms in cholera, and that of poisoning, applied the comparison to the particular case, and assuming Dr Mackintosh to be speaking of precise periods of time, interpreted his evidence to mean that it was not uncommon for cholera to prove fatal in two days or in three days. The shorter period was the more natural subject of remark, as it was, in his opinion, the more probable period from the date of the poisoning till death.

If Dr Mackintosh, who still clings to the phrase two or three days, means not a precise period, but, talking in popular phrase, means *any short period of time*, he is in a worse situation than at present, for he is then exposed to the charge of using ambiguous

and loose expressions, calculated (however unintentionally) to mislead, by their being, as a matter of course, applied to the special case.

It may be proper to state, that, giving Dr Mackintosh the benefit of the longer period, viz. three days, I consider his opinion to be still equally incorrect, cholera indeed seldom proving fatal at all in this country, particularly in its first stage.

VI. I come now to the last and most important point of difference between us. In the reports of the Dundee gentlemen and myself, it is stated that the stomach and intestines were in a state of good preservation, although the external surface of the body was a good deal decayed; and our evidence bears, that, in consequence of that preservation, we were enabled to remark in the stomach certain signs of violent irritation, such as arsenic sometimes produces. Dr Mackintosh deposes, that, "barring the evidence of arsenic, he did not consider the morbid appearances * worth one farthing, because every medical man knows the *post mortem* appearances lose their character in a few days;" and he now wishes "it to be distinctly understood that it was his opinion at the trial, and it is his opinion at this moment, that the appearances found in any body three or four weeks after death are not worth one farthing."

I shall discuss this point at some length, because it is really a very important one in a medico-legal point of view, and the chief facts relative to it are very little known in this country. The subject is one I have had in view for some time, with the intention of making experiments on it. I am sorry I am obliged to bring it prematurely forward without the personal researches I contemplated.

In his reply Dr Mackintosh expresses his doubts regarding the antiseptic properties of arsenic, and even says "other people assert, on the contrary, that there is a putrescent tendency in the bodies of those who die from arsenic." This is very true. But he would have acted wisely if he had consulted some *direct* authority, and not confined his inquiries to the work of Dr Paris, who does not speak from experience, and who, I must take the liberty of saying, does not seem to have been aware, any more than Dr Mackintosh, of the valuable observations that have been lately made on this subject in Germany.

I shall not enter at full length into the subject here: I shall not attempt to ascertain under what conditions arsenic acts as an antiseptic,—for it does not always act as such; but will confine myself to some facts which will prove that it is sometimes a preservative from putrefaction.

I need hardly say that arsenic is used for preserving the skins of birds and beasts. *Guyton-Morveau*, the celebrated French che-

* In the report of the trial were the words "described by the Dundee doctors." They are left out, being objected to, and of no consequence. I took great pains to secure the accuracy of the sentence, and my impression still is *that they were in Dr Mackintosh's answer.*

mist, made some express experiments, and arrived at the conclusion, that arsenic is a powerful antiseptic for articles to which it is immediately applied. Even the solution is antiseptic. In an experiment, which I have made among others with a view to publication, a portion of an ox's stomach has been kept for three years in a solution of arsenic without undergoing any change, except that it has become whiter and somewhat shrivelled.

To come, however, more directly to the point, namely, to the effect of the poison in preserving from decay the bodies of those poisoned with it.—In the case of a man who was poisoned with arsenic, and whose body was kept *nine* days before it was buried, and disinterred and examined *seven* days after that by the late Professor Metzger of Königsberg, in his day the greatest medical jurist in Germany, “the body, to his astonishment, was so fresh, that he could not discover any sign of putrefaction outwardly or inwardly.* A case somewhat parallel was one of those which form the subject of my paper on arsenic in the Edin. Med. Chir. Trans. The body was examined *seventeen* days after death; and the morbid appearances, as will be seen hereafter, were as distinct as in a body recently dead.† In a case of poisoning with arsenic, described by Bernt, now Professor of medical jurisprudence at Vienna, the examination was not made till *five weeks* after burial;—yet, although the external surface was a good deal decayed, “the bowels, to the astonishment of the inspector, were found in as fresh a state as if the man had been but a few days dead.”‡ The same conjunction of circumstances occurred in a late case examined by Dr Borges, medical inspector for Minden: although the body had been buried *fourteen weeks*, and although the external surface had undergone the adipocirous putrefaction, and most of the abdominal viscera were reduced to a pulp, the stomach and intestines were firm, of a grayish-white colour, and contained evident crumbs of bread. || A very remarkable set of cases of the same kind became the subject of judicial inquiry at Munich in 1809–11, and have been related by Dr Bachmann, medical inspector at Culmbach. A lady near Bayreuth died apparently of some disease of the intestines. But afterwards, a variety of circumstances having excited a suspicion that she had been poisoned by her maid, a judicial inquiry was set on foot; and in consequence the maid came under suspicion of having also poisoned first another lady, and afterwards a gentleman, with whom she had been successively a servant. The bodies were examined *five, six, and fourteen months* after interment. The external parts had in all undergone the adipocirous putrefaction; but in the two last of them the stomach and intestines were so entire as to allow of their being tied, taken out, cut open, handled, &c.—

* Pyl's Aufsätze und Beobachtungen, aus der gerichtlichen Arzneiwissenschaft, vi. 96.

† Med. Chir. Trans. ii. 284.

‡ Bernt, Beiträge zur gerichtlichen Arzneikunde, iv. 219.

|| Kopp's Jahrbuch der Staats-arzneikunde, ii. 226.

In two of them, the first and the last, arsenic was detected.* Another similar set of cases, which occurred at Berlin in 1803, gave rise to several interesting essays and express experiments, and first drew the attention of the German medical jurists to the subject. The widow of a certain Privy-Councillor Ursinus, having attempted to poison her servant, suspicions arose regarding the sudden death of three persons in her family,—a young officer who had carried on an amour with her,—her husband,—and an aunt from whom she derived a considerable inheritance. The husband had been buried *two years and a half* before at Berlin, and the aunt above *two years* before at Charlottenberg. The bodies were disinterred by Dr Welper, medical inspector for Berlin, and found dried up, but not putrid. Dr Welper afterwards got one of his friends to make some experiments on animals poisoned with arsenic; and the results were analogous. In dogs poisoned with arsenic, and buried for *two months*, the flesh and alimentary canal were found *red and fresh, as if pickled*; and though the cellar in which they were buried again was flooded for *eight months* after, the intestines were eventually found entire and red, while all the soft parts of dogs killed by blows, by corrosive sublimate, or by opium, and buried in the same place, were converted into a greasy mass.† Another experimentalist, Dr Kelch of Königsberg, buried in February the internal organs of a man, who had died of arsenic, and whose body had remained without burial till the external parts had begun to decay; and on examining the stomach and intestines *five months* afterwards, he found that the hamper which contained them was very rotten, but that “they had a peculiar smell very different from that of putrid bowels,”—“were not yet acted on by putrefaction,”—“and were still as fresh as when they were taken from the body, and might have served to make instructive preparations.” Nay, “they had lost nothing of their colour, glimmer, or firmness.”‡

These facts, collected chiefly by sworn officers, the most eminent physicians in their respective districts, and in judicial inquiries, will prove sufficiently that arsenic has sometimes a preservative power over the stomach and intestines of persons poisoned with it. And it may be presumed, that in such cases a physician acquainted with pathological appearances would not be apt to mistake them for the pseudo-morbid alterations caused by *decay*, since the latter can have no existence. I shall therefore leave the truth of Dr Mackintosh's *general theorem*, and the accuracy of his evidence, to be decided by the facts and authorities now referred to, and proceed to consider the *particular questions*, Whether the stomach was preserved in the case of Warden? and whether, in its state of preservation, there were any pseudo-morbid changes which could be confounded with the morbid appearances discovered?

* Bachmann, Drei Fälle von Arsenik-vergiftung, &c. contained in a work entitled “Einige auserlesene medizinisch-gerichtliche Abhandlungen.”—Nurnberg, 1813.

† Neue Entdeckungen betreffend die Kennzeichen der Arsenik-vergiftung, &c. Augustin's Repertorium für die öffentliche und gerichtliche Medizin, l. i. 26.

‡ Hufeland's Journal, xix. iv. 111. and xxii. i. 166.

1. As to the former of these questions, it will be remarked in the first instance, that four medical gentlemen, of experience and attainments not inferior to Dr Mackintosh, and who possessed the advantage (in this case not a small one) of having personally examined the stomach, were of opinion, that, at the time they did examine it, it was in a state of sufficient preservation to allow them to distinguish morbid appearances with precision. Add to this the fact sworn to in my evidence, that the stomach was equally entire a fortnight after my first examination was made. And it will be allowed, that, to shake the credibility of our evidence, something more will be required than Dr Mackintosh's *speculative opinion*;—for it will be remarked he does not pretend to speak from personal experience in cases of poisoning with arsenic.

But, in the second place, I am perfectly willing to throw aside these strong collateral circumstances, and meet Dr Mackintosh on the pathological facts of the case.

I shall set out with a short statement of the pseudo-morbid appearances which arise in the stomach in its progress towards decay, because Dr Mackintosh, by obscuring the general subject in his remarks, has contrived to throw doubt over the particular case when really no doubt can exist.

Within three, ten, or fifteen hours after death, the mucous membrane of the stomach, from being of a dull white colour, and free of vascularity, frequently, although quite healthy, becomes partially or generally injected with blood, sometimes to an extraordinary degree. The coloration may consist either of a minute injection of vessels, or of patches of actual effusion of blood in or under the mucous coat. It has various tints between pale rose-red and deep reddish-brown. In the course of my observation, which has been long particularly turned to the subject, I have never seen these pseudo-morbid effusions of a black colour, properly so called,—I have never seen them of the tint of *melanotic* depositions; neither is any appearance of the kind noticed in the works of the most recent and esteemed authors; * but I am informed by my friend Dr Cullen that he has seen melanotic coloration where disease did not appear to have existed during life.—The membrane at this time may be either firm or softened. If it is softened, however, that is not necessarily a sign of decay. It may take place immediately after, possibly also even before death. *If it is firm, decay cannot have begun.*

The first distinct marks of approaching decay in the stomach are general softening of its villous coat, and disappearance of the minuter vascularity, whether morbid or pseudo-morbid. The disappearance of the vascularity is evidently owing to the blood contained in the vessels transuding through their coats. It is followed, or rather attend-

* Billard, *sur la Membrane Muqueuse Gastro-intestinale dans l'état sain, et dans l'état inflammatoire*, 1825.

Rigot and Trousseau, *Recherches Nécropscopiques*, &c. in the *Archives Générales de Médecine*, for October and November 1826.

ed by a general tinging of the whole coats of the stomach with a diffuse, commonly pale, and almost always brownish-red blush, upon which only the larger vessels and darker extravasations are visible. The softening of the villous coat may precede this resolution of vascularity; but I have repeatedly seen the latter precede the former.

About the same time, or soon afterwards, the whole membranes of the stomach become tinged reddish-brown, or greenish, where it lies in contact with the liver, spleen, colon, &c. The subsequent changes it is needless to follow, as they do not bear upon the present case. Besides, they have hitherto been little, if at all examined, as the source of pseudo-morbid appearances.

If the reader bears in remembrance the foregoing statements, and turns to what is said of the appearances in the stomach by the Dundee gentlemen and myself, he will at once see what was its degree of preservation. The state of the external surface of the body, so much dwelt on by Dr Mackintosh, furnishes, as will be seen at once from the cases quoted above, *no presumption whatever of the state of the internal organs.* Drs Ramsay, Johnston, and Taylor, found the stomach, twenty-two days after death, "every where deeply painted with red vessels externally." Five days afterwards, *when*, be it remarked, *I knew nothing of the contents of the Dundee Report*, I found no vascularity in the stomach and intestines, "but a uniformly diffused, light-brownish-red colour, such as these parts assume, if they have contained much blood in their vessels, at the time of death." The "*mere surface* of the villous coat was softened;" but otherwise that membrane was (not as Dr Mackintosh ingeniously insinuates, "softened, so that a mere touch, performed in the gentlest manner, might produce an appearance of ulceration and abrasion"—but) "*very firm*," except on one or two of the dark patches, where it was evidently destroyed. Hence it is evident, that the parts, while in the hands of the Dundee gentlemen, were in a state of complete preservation, and when they reached me were hardly less so. The vascularity had been resolved, but the villous coat was not yet softened.

Lest further evidence should still be called for, I have also to mention that a part of the stomach is in my possession, which was not put into spirits till *six* weeks after death, and has been very carelessly preserved since; yet the mucous membrane *is still so firm that it cannot be removed from the subjacent coats by the nail without considerable force.*

2. We are now prepared to answer the second question in the case of Warden, namely, Whether, in the state of preservation of the stomach, there were any pseudo-morbid changes which could be confounded with the morbid appearances discovered? To this question, it may be at once answered, that, so far as its state of decay was concerned, there was not any impediment in the way of ascertaining what morbid appearances were present; and consequently the censure conveyed in Dr Mackintosh's *evidence* was incorrect.

But his *reply* also gives rise to a question, whether there were such appearances present as indicated,—not poisoning with arsenic,

for no such opinion was ever attempted by any of the crown witnesses to be founded on the morbid appearances alone, but—inflammation or irritation of the stomach, as distinguished from those imitative appearances which arise soon after death and *before decay begins*? I shall simply answer that question, by recapitulating the appearances actually found. The stomach was red and vascular; its villous coat was corrugated, in some parts separated, in some irregular patches removed altogether, in other broad patches elevated, and (not “of a deep red colour,” as Dr Mackintosh again ingeniously alters my description, but) of a coal black or melanotic colour; at some points little agglomerated particles of powder were “deeply indented into its substance,” and surrounded by a mark like that of corrosion; over the stomach and intestines, “a white powder, in most minute particles, was every where distinctly seen, enveloped in a thick and generally bloody mucus;” and the contents of the stomach consisted of a thick brownish red fluid, which was, in part, altered blood.

In considering this description, the reader will of course avoid Dr M.'s example, when he views each item separately from the rest, corrupts the descriptive language of some, and omits others altogether. He will consider them in conjunction with one another; he will consider them as occurring along with great firmness of the villous coat where it was not actually removed; he will consider, particularly, the *imbedding of the agglomerated masses of powder in deep cavities of the membrane*; and he will then say whether Drs Ramsay, Johnston, Taylor, and myself, are not borne out in our opinion, notwithstanding the speculations of our opponent.

Dr Mackintosh, if I mistake not, seems to imagine we stand alone in our opinion,—that the appearances caused by arsenic may be sometimes distinguished after a long interval. But I am sorry that neither the gentlemen of Dundee nor myself can claim the merit of a discovery of so much importance to medical jurisprudence and the administration of justice.

In a case I have related elsewhere, the reporter remarks, “The whole outer surface of the stomach was vascular and of a dark livid colour, bearing strong marks of recent and violent inflammation.”—“The inner coat was found streaked with broad patches of a dark livid colour.”—“Although seventeen days (in September) had intervened from the time of his death till the day on which the body was examined, yet, from the comparatively slight degree of decomposition that had taken place, as well as from the great contrast between the morbid appearances of the stomach and intestines, there can be no hesitation in saying that violent inflammation of the former may be considered as the immediate cause of death.”*

In Professor Metzger's case, formerly quoted, the appearances were still more similar to those in the case of Warden. “The inner surface of the stomach was checkered with many gangrene spots;† the rugæ were much swelled up, like thick cords; the sub-

* Ed. Med. Chir. Trans. ii. p. 284.

† See note † next page.

stance of the stomach itself was thickened, yet without any separation of the villous coat. So exactly was the duodenum," &c. And the Professor does not hesitate to pronounce the said appearances, those of "violent inflammation of the stomach and intestines."*

In the case I have quoted from Professor Bernt's Contributions, — a case in which the body had been buried five weeks, the inspector found, "towards the cardia, several large, black, mortified, † elevated, knotty patches, which extended in rays along the coats of the stomach;" and Professor Bernt, one of the ablest medical jurists now living, designates the whole case as "an additional and quite impartial proof of the antiseptic powers of arsenic, and a fresh evidence that *interment and decay do not always frustrate the object of medico-legal inspections.*‡"

In one of Dr Bachmann's cases, which was examined after six months' interment, he found "in the region of the pylorus a slough† (*smegma*,) under which the villous coat had a brownish-black colour;" and in his remarks on the evidence of poisoning, he never dreams, like Dr Mackintosh, of mentioning the decay of the *rest of the body* as any impediment to the conclusion, that the *stomach*, which was entire, had been subjected to violent irritation.§

In the case experimented on by Kelch, "the inflamed spots (on the stomach) had not disappeared" after five months interment; and the small intestines showed "in some places the spots of inflammation with the redness unaltered." ||

I must not dismiss these interesting documents, however, without cautioning the reader, that I am far from agreeing with the describers of the cases, that marks of inflammation were unequivocally found in all, and still farther from thinking, that in any of them such unequivocal proofs of inflammation were found as in the case of Warden. I quote them chiefly to show, that, among the experienced and learned men who conducted these investigations, no one ever imagined, that, when the organs he examined were in a state of good preservation, *there was any risk of his confounding the appearances seen with the effects of decay, merely because the body had been long buried.* The fallacies produced by those pseudo-morbid changes, which occur *before decay begins*, do not seem to have been attended to by them. They have indeed been but lately made an object of study, and are still not generally known.

From the preceding observations, the profession will sufficiently appreciate the value of the statements in the reports, when they describe the morbid appearances in Warden's case.

Dr M. in conclusion has made it a subject of complaint, that I alluded without his leave to a conversation explanatory of his evi-

* Loc. Cit. p. 99.

† Most probably in this and the two previous cases, the supposed gangrene was nothing else than the peculiar black extravasation and disorganization, such as occurred in Warden's case. A similar mistake was committed by John Hunter in putting up a preparation in his museum.—Sec Brodie on Poisons, Phil. Trans. 1811-12.

‡ Loc. Cit. p. 219.

§ Loc. Cit. 22.

|| Loc. Cit. 115.

dence, which took place between us immediately after he left the witness-box ; and he hints, that, as he turned but one ear to me, and the other to the court, he does not know what he may have said. I am satisfied his words were too explicit to bear any other meaning than I attached to them ; and I saw no impropriety in publishing a statement *advantageous* to him,—being an explanation which virtually ascribed to inaccuracy of language, or the confusion of the moment, what otherwise could be ascribed only to his being little acquainted with the subject of which he spoke.

Nothing shall tempt me to institute any comparisons to Dr Mackintosh's disadvantage, or to decry his general experience. But I must own, his notions as to the experience required to authorize observation, or to form an opinion on a particular case, appear not a little rude and unscientific, when, implying the necessity for such previous knowledge, he asks how many stomachs I have examined three weeks after death, which had been packed in a pill-box, and carried sixty or seventy miles by a mail-coach ?

This query may be answered by an illustration. " Let us suppose that a savage," says Mr Dugald Stewart, while treating of the influence of *Casual* Associations on our speculative conclusions,—“ Let us suppose that a savage, who, in a particular instance, had found himself relieved of some bodily indisposition by a draught of cold water, is a second time afflicted with a similar disorder, and is desirous to repeat the same remedy. With the limited degree of experience which we have here supposed him to possess, it would be impossible for the acutest philosopher, in his situation, to determine, whether the cure was owing to the water which was drunk, to the cup in which it was contained, to the fountain from which it was taken, to the particular day of the month, or to the particular age of the moon. In order, therefore, to ensure the success of the remedy, he will very naturally, and very wisely copy, as far as he can recollect, every circumstance which accompanied the first application of it. He will make use of the same cup, draw the water from the same fountain, hold his body in the same posture, and turn his face in the same direction ; and thus all the accidental circumstances in which the first experiment was made, will come to be associated equally in his mind with the effect produced.”*

On the same principle, that, in investigating the virtues of the fountain, I should discard the posture, the direction of the body, and even the cup, it appears to me, with all possible diffidence, that the number of miles which the stomach had travelled was not a material circumstance in the examination ; that the mail-coach was not an indispensable element ; and that the box itself, in which the stomach was placed, might also be safely left out of view. “ It is the enlargement of experience alone,” continues Mr Stewart, “ which can free the practice of medicine from *those superstitious observances.*”

Dr Mackintosh has put a question to me which I have *candidly*

* Elements of the Philosophy of the Human Mind, i. 348.

answered ; I shall, in my turn, put a question to him, which perhaps is not equally material, but which may have some slight bearing on the case : How many stomachs has he examined where death has been caused by arsenic ? Did he ever examine *one* ?

ART. XII.—*Remarks by Dr C. I. B. WILLIAMS addressed to the Reviewer of his Essay on Respiration and Animal Heat noticed in this Journal for January last.*

SIR,

I HAVE not until now had an opportunity of reading your critique* on my Essay on Respiration and Animal Heat in the Transactions of the Medico-Chirurgical Society of Edinburgh, (Vol. ii.) and whilst I offer my acknowledgments for the generally favourable opinion which you have passed upon it, I hope you will allow me to show that the individual parts which you have deemed objectionable, at least involve neither “logical inconsistency” nor “important oversight.” I am the less surprised that my expressions have been misunderstood ; as the essay was condensed into a space too narrow for a full and clear explanation of the theories, and some important points were entirely left out. If you will do me the favour to insert a few explanatory observations in your Journal, these lacunæ will be in some measure filled up.

You object to the theory of animal heat as inconsistent with that of respiration. “Dr Williams contends, in his reasonings on respiration, that carbonic acid exists in the blood ready made, and is simply displaced by the oxygen of the inspired air. In his speculations on the source of animal heat, on the other hand, he is obliged to have recourse to the *opposite* hypothesis, that there is an incessant combination of oxygen with carbon in the living body.” Now, so far is the latter from being *opposed* to the preceding position, that it is an essential part of the same theory. The conversion of blood, from the venous to the arterial state, I conceive to be effected by the displacement of some of its carbonic acid by oxygen. Arterial is reconverted into venous blood by the union of the absorbed oxygen with carbon in the course of the circulation. It is thus that the carbonic acid, which experiment has detected in blood, is formed. It is by exposure to the air in respiration that its excess is removed, and that oxygen is absorbed to fit the blood for its purposes in the body. The presence of carbonic acid in the blood, and the

* See Number for January 1827.

union of carbon and oxygen in the living body, are then not only consistent with each other, but stand in the relation of cause and effect.

The question as to the source of the carbon, which, uniting with the oxygen of arterial blood in the course of the circulation, changes it to venous, I have simply answered in my essay, referring to my Thesis* to substantiate my opinion; and I shall here merely state, that it was from a comparison of the ultimate composition of the various animal principles, according to the most accurate analyses, that I was led to conclude that the assimilation of chyle *cannot*, and that the secretion of urine, saliva, &c. does, leave an excess of carbon in the blood, which may supply the demand of the respiratory function.

But I apprehend it is not the most physiological view to take this secretion as the object. I would place the changes of the blood in this light:—The albuminous matter of oxygenized blood, obeying chemical affinity, modified in an uncertain manner and degree by living structure, becomes gradually changed into new and simpler matters. The oxygen, by uniting with a portion of the carbon of the albumen, becomes carbonic acid, leaving animal principles containing less carbon, namely, urea, mucus, &c. In fact, the new principles, carbonic acid, urea, mucus, picromel, &c. are the products of new affinities discovered by the absorption of oxygen in arterialization, by the unknown influence of living structure, or more probably by the joint power of both. Here the theory might be illustrated by an infinity of phenomena, physiological and pathological, but these I leave for the ingenious reader to supply.

You will now, perhaps, more easily perceive why the liquid secretions tend to the support of animal heat; but the full force of the opinion cannot be felt until the law, of which it is an example, is recognized, and to it I claim the attention of chemists.

It would lead me too far astray from the temple of Hygeia, were I to examine all the instances where the reduction of ternary and quaternary compounds into compounds simpler, and of fewer atoms, is accompanied by the evolution of heat; suffice it that I adduce some marked examples. The most efficient cause of heat by chemical change is the union of two contrary elements, with no opposing affinities to restrain their combination: Such is the union of oxygen and hydrogen. The next may be the combustion of animal and vegetable matters; the simplest compounds result; but the calorific process is somewhat impaired by the pre-existing slight affinities. A lower degree of heat, but yet luminous, is produced by a slow combustion, which converts

* Diss. Chémico-Physiologica de Sanguine ejusque mutationibus, 1824.

many compound animal and vegetable matters partly into water and partly into an empyreumatic acetic acid, both more simple in atomic composition than the combustible. The burning of the vapour of ether or alcohol by means of a platinum wire is an example; but I have shown that the process is much more general, and may be exhibited in oil, wax, and most other compound inflammables at a heat below redness.* Lastly, I come to the lowest cases of calorific union, fermentation and putrefaction. In one, sugar is converted into alcohol and carbonic acid, alcohol into acetic acid, &c. In the other, animal or vegetable compounds are converted into simpler,—ammonia, carburetted hydrogen, carbonic acid, &c. In all these cases it may be observed, that, *cæteris paribus*, the evolution of heat is proportionate to the change from isolation or weak combination to firm and simple union. Now, that the formation of the principles of the fluid secretions, as urea, mucus, picromel, &c. from the albumen and fibrine of the blood, is attended with a production of heat according to this law, may be presumed, inasmuch as it is an instance of the conversion of compound into simpler matter.† The fact that animal heat bears a uniform relation with the activity of the secretory function, has been frequently recognized by experimental physiologists;‡ and I might adduce many other instances to establish it, were it called in question.

If I have not given “the molecular process of nutrition” as a cause of animal heat, this is no proof that I have overlooked it. Had I mentioned it, it would have been negatively; for according to no known law can I see that such a process can prove a direct source of animal heat. All speculations about the conversion of fluids into solids may be met with the fact, that the reverse equally takes place, and may be proved inefficient on the axiom, “if equals be added to equals,” &c. To have formed a mere hypothesis of the production of heat in an unknown manner would have been inconsistent with the tenor of my essay, the object of which was to explain physiological facts by referring them to known physical, chemical, and vital laws. Moreover, I find full reason to question that nutrition is essentially, and in itself, a source of animal heat. Many facts might be adduced to show that, however subservient secretion may be to nutrition, it is to the former that the evolution of heat is to be ascribed. Take for example the young of animals. In them nutrition is most active, while the secretions are both less abundant and less characteristic than in those full grown:—and yet their

* Vide Annals of Philosophy for 1823.

† See the Analyses in Thomson's Chemistry, Vol. iv. pp. 407, 412, 418, 421.

‡ Brodie, Dissert. &c.

temperature is lower.* In adult age, when growth is stationary, it is doubtful whether secretion can be considered entirely subservient to nutrition. Physiologists have contented themselves with the vague assertion, that even then the body is in a state of continual decay and reparation. I suppose that by decay is meant the resolution of its animal matter into simpler products; and that by reparation is meant the deposition of new animal matter from the blood. The former is then a part of secretion; the latter constitutes nutrition. If the former did not take place, there would be no requisition for the latter. But decay does take place, (it is supposed,) and reparation must follow; and we are reduced to the dilemma,—Is decay the necessary result of a defective composition? or is it, as supplying secretion, part of a useful and important function, that of furnishing heat to the body? Who is there that has studied the beautiful and perfect economy of nature, that will hesitate between these alternatives?

Neither I, nor even Bichat, can lay claim to the originality of the opinion, that the generation of animal heat takes place, not in the lungs, but in the capillary system; but I believe neither Bichat, nor any other author, has pointed out the reasons that I have shown *why* the capillaries, and particularly those of the secretory organs, should be more especially the seat of its evolution.

These explanations I have deemed it my duty in self-defence to give, and although I think that a careful examination of my essay would render most of them unnecessary, I beg that you will give your readers an option of their perusal. I have the honour to be, Sir, your most obedient servant,

CHARLES I. B. WILLIAMS, M. D.

*Golden Square, London,
April 26, 1827.*

* Edwards, De l'influence des agens physiques sur la vie.

PART II.

CRITICAL ANALYSIS.

ART. I.—*Memoires ou Recherches Anatomico-Pathologiques, &c. &c.*—*Memoirs, or Anatomico-Pathological Researches on various Diseases.* By P. CH. A. LOUIS, M. D. of the Faculties of Paris and St Petersburg, &c. &c. Paris, 1826.

TO those who are acquainted with the periodical publications of the French metropolis, it is unnecessary to say, that M. Louis has for some years past been an active contributor to some of the best of them. Six memoirs in successive numbers of the *Archives Generales*, one in the *Revue Medicale*, and two in the Repertory of M. Breschet, are the fruitful proofs of his diligence in acquiring and communicating precise information. As further inquiry has since enabled him either to enlarge, to correct, or to confirm his former conclusions, he has been naturally led to publish these essays in a collected form, and to make such modifications and additions as maturer experience suggested. To these are now added three memoirs, which have not yet been published ; and which, though not forming a very large part of the volume, are in every respect deserving the attention of the pathologist.

It is superfluous to offer any apology for bringing the contents of the present volume more immediately under the notice of the pathological student, than they formerly were, when confined to the pages of the journals in which they originally appeared. The researches of M. Louis have not merely extended the boundaries of pathological knowledge. They have added very considerably to its precision ; and, by the careful comparison of many cases, have tended much to the establishment of correct general results. It is chiefly to these that the attention of the medical reader will be most profitably directed.

Softening and Extenuation of the Mucous Membrane of the Stomach.—Of the first memoir in the volume, the subject is a severe morbid change in the consistency and thickness of that membrane, which, though so frequent as to be found in $\frac{1}{2}$ of

all the subjects cut off by chronic diseases of various kinds, nevertheless has not been described with any accuracy.

This change is chiefly known by some part of the inner or mucous surface of the organ having assumed a pale white, a bluish, a rose, or a grey colour, continuous, or disposed in long narrow bands, or irregularly rounded patches more or less thickly set. The spaces so coloured are not on a level with the contiguous parts; but the boundary lines, instead of being neatly and perpendicularly cut, as in ulceration, are simply depressed, while the discoloured membrane itself, so limited, is extremely soft and thin, and appears to be converted into a sort of glairy semi-transparent pulp, not thicker than the mucous membrane of the colon. The space occupied by this change, which indicates a real destruction of the membrane, was in some instances small, in a few extensive. When it occurred in large narrow stripes, it was almost equally distributed over the whole surface of the stomach; when, on the contrary, it was continuous, it occupied the large extremity of the organ, was rarely confined to the great *cul-de-sac*, and in some instances appeared near the pyloric and cardiac orifices at the same time.

The vessels distributed at the surface of the submucous tissue were very distinct, large, and empty. In sixteen cases particularly examined, the submucous membrane itself was, with two exceptions, sound, and of the ordinary firmness; and in the same cases only was the muscular layer softer than natural.

The nature of the change thus briefly sketched may be understood, from the circumstances in which it is observed to take place, from the changes in the physical properties of the gastric tissues with which it is attended, and from the progressive effects which may occasionally be observed in certain cases, in which it may be traced in different stages of advancement. Little doubt can be entertained, that it is the result of an inflammatory process, affecting certain points of the mucous membrane, and proceeding apparently from its free to its attached surface, and thence to the subjacent tissues. Thus the various shades of colour, which the gastric membrane assumes, doubtless indicate a process of inflammation in a stage more or less advanced. In four cases it was of a rose colour, more or less intense; in three it was greyish; in one it was greenish or bluish white; and in some cases this red tint was particularly brilliant in the vicinity of the extenuated portion of membrane. It is with some justice remarked by M. Louis, that the colour varies so much, that no certain inference can be deduced from its presence, its kind, or its intensity. But this very circumstance of variation is manifestly dependent on the stage of the diseased process, and, when taken along with other circumstances, affords very strong evi-

dence of the nature of the change. Thus the irregular, papillated, and granular appearances remarked in four of the cases are very probably, and the ulceration more or less deep is certainly, to be regarded as the result of inflammation. But even on the supposition of these effects being equivocal, the progress which may in certain cases be traced from the slightest to the most complete destruction of the gastric tissues, must doubtless be admitted as satisfactory evidence of the nature of the process. Thus, M. Louis observed, in the same subject, extenuation, with slight softening, extenuation with considerable softening, destruction of the mucous membrane, and destruction of all the tissues to a certain extent, except the peritonæum. In such circumstances, he justly remarks, the pathologist has an opportunity of observing the successive changes, from the first and slightest degree of softening to complete perforation of the organ.

Nor is the information furnished by those external changes in the appearance, sensations, and functions of the individual, which constitute so many symptoms of the internal disease, to be altogether disregarded. In the majority of cases, digestion was more or less impaired, in some for a series of months, or even years. The appetite was variable. In all the cases there was squeamishness, and twelve of them had vomiting. In eleven of the twelve fatal cases, and in two of three favourable ones, the epigastric region was more or less painful, sometimes acutely so, sometimes accompanied with a sense of weight, of obstruction, of heat, or of some other unnatural sensation. These epigastric pains, however, were not constant; and, while they were evidently relieved at certain periods of the disease, at others they were much aggravated; and then the countenance, which was at other times natural, was invariably expressive of great uneasiness, anxiety and distress, not dissimilar to what is remarked during colic.

The effect upon the general strength varies, and is not easily estimated, by reason of the morbid states of the system, with which the affection of the stomach is generally complicated. In some instances it appears to be inconsiderable, as in one case the patient continued her usual labours to the 12th day previous to death, and the subject of another to the 3d day from the fatal event. In other instances in which the progress of the disease is rapid, as indicated by vomiting and copious diarrhœa, the decline of the strength is speedy.

The duration of the complaint was in general considerable, and its progress therefore slow. Essentially chronic, when in complication with diseases more rapid in progress, its duration was at least from $3\frac{1}{2}$ months to 6, and to 13 months, without be-

ing connected with a corresponding change in the organs after death. In other cases, however, which are doubtless less frequent, it assumes the character of an acute disease, and may run its entire course in a space of from 20 to 40 days. Its natural tendency appears to be towards death; and it is only when its progress is either retarded or arrested by suitable measures, that recovery appears ever to have taken place. It is scarcely necessary to say, that the possibility of this favourable issue must depend mainly on the stage of the complaint, and on the degree to which the gastric mucous membrane has been injured. M. Louis gives three or four cases of the disease, in which recovery took place under the use of mild diet, vegetable ptisans, and the occasional application of leeches to the epigastric region.

The prevalence of this disease may be understood from the following statement. Among 220 persons cut off by various chronic diseases, from the three concluding months of 1822 to the date of the publication of the present volume in 1826, in the wards of St John and St Joseph, 33 presented softening and extenuation, more or less extensive, of the gastric mucous membrane;—21 among females, and 12 among men. It was somewhat less frequent among persons cut off by acute diseases, being 12 times in 115 cases. It was latent or unattended by obvious symptoms in a sixth part of the cases of fatal chronic diseases.

Besides the morbid change now described, M. Louis has observed in the bodies of persons cut off by the most opposite diseases, another state of the gastric mucous membrane, which has not yet been described. This consists in some part of the membrane presenting eminences various in shape and size, but in general round, and from 2 to 3 lines in diameter, like ordinary granulations, separated by superficial furrows,—and one or more points of ulceration, round or oblong, from one to several lines in diameter. The colour of the membrane is then changed, sometimes to red, more frequently to a greyish tint; it is in some cases softened, and almost invariably it is thicker than in the sound state,—a circumstance by which it is particularly distinguished from the sort of lesion already mentioned.

This increase in thickness is sometimes so remarkable, as at once to attract the attention by reason of the general swell by which the outline of the part is marked, and separated, as it were, from the contiguous parts. The villous membrane in this state is generally covered with much viscid mucus.

This granular state of the gastric membrane, which is observed in the most opposite circumstances, is most frequently observed in the large curvature, and the parts adjoining to the anterior and posterior surfaces, at the pyloric extremity, the small curva-

ture, and the great *cul-de-sac*, which, however, is very rarely granular in its whole extent.

This state of the gastric membrane is justly ascribed by M. Louis to inflammation. As it has rarely been seen without complication, and appears indeed to be one of those conditions met with in the persons of the phthisical, it is not easy to ascertain the symptoms to which it gives rise. In most of the cases in which it was found, the appetite was impaired, either from the commencement or in the course of illness; squeamishness and vomitings took place occasionally; and in one only of nineteen persons was the epigastric region tender.

Hypertrophy of the muscular membrane of the stomach in its whole extent forms the subject of the second memoir. This term is applied by M. Louis to a hardened, thickened, and somewhat reddened state of the muscular layer of the stomach, which is found to take place during that process of chronic inflammation which attends cancer and other lengthened organic affections of the organ. The ordinary effect of the carcinomatous ulceration is here manifested in rendering the contiguous tissues, and especially the muscular, exceedingly firm and consistent; and in augmenting its thickness to the extent of one and a half lines or two lines, in the neighbourhood of the cancer, and to about half a line round the great *cul-de-sac*, which is fully double the thickness of this layer in the natural state. In one of the two cases mentioned by M. Louis, the colour of this tissue was not changed. In two instances which fell under observation, however, the muscular fibres were brownish near the seat of ulceration, and bluish red in those more remote; and it is worthy of being noticed, that in the cylindrical portion of the organ in the present case, M. Louis afterwards admits, that the muscular coat was *bluish*, with a shining semitransparent tint. The induration is not confined to the muscular layer, but is very conspicuous in the submucous tissue, which is described as grating under the knife.

Hypertrophy of the gastric muscular tissue thus described, M. Louis is inclined to attribute to contraction of the pyloric end of the organ, precisely as hypertrophy of one of the cavities of the heart is referred to some degree of obstruction to the issue of the blood. But he admits a serious objection in the fact, that, though cancer of the stomach is frequent, hypertrophy is rare. Certain facts favour the opinion, that the alleged rarity depends on defective or careless observation; and it may be asserted, without much chance of error, that, as cases of gastric cancer are more accurately examined, the change in the muscular tissue remarked by M. Louis will be more frequently and generally recognized. It must also be borne in mind,

that the extent to which it takes place, and its degree of perfection, must be regulated very much by the stage of the cancerous action. Thus, while the disease is still confined with more or less accuracy to the mucous membrane or its follicles, as it is in the incipient stage, and the submucous tissue still affords a sort of barrier to its progress, the muscular layer will be little if at all affected. When, on the contrary, the disease has continued long, and the process of destruction is attended with more or less of the usual but morbid efforts at repair, the muscular layer is no longer exempt, and will suffer in proportion to the extent and violence of the cancerous action.

The situation of the scirrhus part is also a circumstance, the influence of which in contributing to the effect in question is by no means inconsiderable. A scirrhus point situate near the pylorus both more speedily and more severely produces its effects on the adjoining parts and textures than one more remote.

Before concluding these remarks, it seems proper to say, that the term which M. Louis, doubtless after the example of most of his countrymen, has chosen to designate this change is not extremely appropriate. Though the muscular tissue is doubtless rendered thicker, more bulky, more dense, and more firm, it is certainly not so by excess of nutrition. The individual fibres are slightly enlarged; but they are also changed entirely in structure. Some of the increased thickness is also due to the connecting cellular tissue, which is invariably loaded with fluid, and the minute vessels of which seem to be inordinately distended. The muscular tissue, in short, when hypertrophied, seems to be in a state of chronic inflammation, of which the increased thickness, the firmness, and the change of colour are at the same time so many effects and indications; and this view, it may be observed, is more consistent with correct pathology than that implied in the appellation given by M. Louis.

Perforation of the small intestines during acute diseases.

—The subject of the third memoir is already in some degree known to the readers of this Journal, in consequence of some observations on it made by our correspondent Dr Abercromby, in illustration of his remarks on the pathology of the mucous membrane of the stomach and bowels. (Vol. xxi. p. 239.) Though this is one of the most severe accidents which the physician can encounter in the practice of his art, and gives rise to most frightful symptoms terminating fatally, yet it has not particularly attracted the attention of observers; and the memoir of M. Louis is the first systematic account of it which has been laid before the profession. His description, which is the result of the observation and dissection of ten cases, eight of which occurred at La Charité, under the care of M. Chomel, and the

other two were communicated by two friends of the author, may be shortly comprised in the following sketch.

The perforation took place in all the cases in the last twelve inches of the ileum. Though double or treble in two cases, it was in general single, and always in the centre of a patch of ulceration. The intestinal mucous membrane traced from the duodenum to the cœcum, presented oval patches more or less prominent, then ulcers more or less ample and deep, perforated or unperforated, and situate like the patches at the anti-mesenteric part. These ulcers varied according to the depth to which the ulcerative process had penetrated; and while in some the mucous membrane was more or less extensively destroyed without affection of the submucous tissue, in others the latter already softened showed the beginning of the disorganization, or was much or entirely removed. It was chiefly in the ulcers situate near the lower end of the bowel that this process was furthest advanced; for in these the mucous and cellular tissues had disappeared to the same extent; and the muscular layer, which was then generally red, and in some cases a little thicker than usual, was completely exposed. The margin of these ulcers was more or less ample and angular, thin, or prominent, according to the kind of patches which had preceded them. Lastly, in the centre of those ulcers in which were perforations, the muscular and peritoneal coats were destroyed to the extent of from one to two lines; and, generally speaking, less of the peritoneum was deficient than of the muscular layer. From the sharp, clean margin of these ulcers, and from the want of the gangrenous appearance or smell, M. Louis infers that the perforations were in a great degree the result of absorption.

Inflammation was the primary cause of this sort of injury. M. Louis traced the successive stages of the process from those patches of the intestinal mucous membrane which were more or less red, thickened, and pulpy, to those in which ulceration had commenced; next to those in which the mucous membrane was destroyed, when its edges were red, soft, and pulpy; and finally, to those in which the submucous tissue was removed, and the muscular exposed or completely destroyed. These changes, however, did not seem always to occur in the same order; and in some few instances the affection of the mucous membrane appeared to follow a previous affection of the submucous tissue.

The effects of perforation, which are important to be known, are the following. 1st, The peritoneum is at once covered by a false membrane, and acquires a red colour more or less deep; 2d, It may in this state be easily detached without being broken; 3d, The fluid effused into the abdominal cavity, contain-

ing a certain quantity of matter proceeding from the ileum, possesses characters different from that which is the product of ordinary peritonitis, and a peculiar odour, which is occasionally sufficient to denote perforation of the bowel, in subjects in whom it had not been observed during life.

The circumstances under which this accident took place, and the effects which it produces, deserve notice.

The persons in whom it occurred were young, and in the vigour of life. With one exception, their constitutions were good; they had rarely been unwell, had not committed any excesses, and presented the characters of the sanguine bilious or lymphatic sanguine temperament. All of them had recently come to Paris.

With the exception of one case, the disease began like a slight continuous fever, and presented no severe symptom till the period of perforation. Some degree of diarrhœa appears to have existed in all the cases; but in general this was so readily controlled by remedies, and any other symptoms were so mild, that it must have been impossible or absurd to apprehend any severe result. So correct is this as a general conclusion, that M. Louis admits, that, with the histories of several examples of intestinal perforation before him, he should not in any analogous cases be capable of fixing on any symptom which might distinguish them from those terminating in speedy recovery.

In the absence of more positive evidence of the commencement of these ulcers, it is not unjust to infer that it coincides with the first symptoms of illness; and this would show the progress of these ulcers to be sufficiently rapid to attain their final term within a space varying from 12 to 25 days, rarely longer. This event might be conjectured by the patient's experiencing all at once in the belly an intense tearing pain, aggravated by pressure, quickly followed by decomposition of the features, nausea, vomiting, &c. and which continuing with more or less violence from 20 to 54 hours with remissions of various length, denoted very intense peritoneal inflammation, produced by the sudden operation of a violent cause.

The diagnosis of this accident, however, is by no means at all times easy or certain. Independent of the circumstance of acute pain of the belly occurring without peritoneal inflammation, and of peritonitis itself taking place again without perforation, in the course of several diseases, a peculiar difficulty results from the patient's being occasionally attacked with delirium, which most effectually disguises the other more direct symptoms. To obviate the source of the first fallacy, it is indispensable to remark, whether the pain is affected by pressure; whether it has come on suddenly, and increases progressively with nausea

and vomiting; and, if it abates at intervals, which occasionally happens, whether the anxious contracted expression of the countenance and the vomiting continue. To counteract the effect of the second source of confusion, it is important to attend to the situation and direction of the pain. In ordinary peritoneal inflammation, the pain is generally not confined to any particular spot. In that resulting from perforation, it is at least in *some* instances, and at the commencement of the action, referred in general to the situation of the cæcum, and seems to proceed with more or less accuracy from the vicinity of this bowel. With all these precautions, however, the observer meets with cases in which no acuteness or sagacity can positively ascertain the occurrence of this accident. When this uncertainty results from the symptoms being masked by delirium, M. Louis thinks that, if the patient winces under strong and continued pressure of the belly, and presents other symptoms, perforation may be suspected.

The interval between the occurrence of the first symptoms which may be conceived to denote the accident of perforation, &c. and their termination in death, varied in the cases observed by M. Louis from 20 to 54 hours; and, generally speaking, the most delicate subjects perished in the shortest time. In one instance, which is to be regarded as an exception, the individual survived not less than seven days from the date of the symptoms of perforation, though they were violent when they commenced, and did not abate till the beginning of the fourth day.

It can scarcely be supposed that any medical treatment can have much influence on such an accident as perforation of the intestine. The peritoneal inflammation, which results from the escape of the contents of the bowel into the abdominal cavity, cannot be controlled so long as foreign substances remain in this cavity; and all that human aid can effect in such circumstances is chiefly of a passive nature. The only means which nature has provided against the effects of perforation, are adhesion, by means of the effusion and organization of coagulable lymph; and in some rare instances, perhaps, it is not impossible that life may be prolonged by the operation of this power. The first and most obvious measure, therefore, where perforation is either apprehended, or is supposed to have already taken place, is perfect rest, and leaving the bowels as free from motion as possible. The second is the employment of those means which are known to control peritoneal inflammation, or to mitigate the severity of its attendant symptoms. Local bleeding by leeches, emollient fomentations, and anodynes, are the chief remedies recommended by M. Louis for this purpose.

The fourth memoir is on the subject of *croup*, as it appears

in the adult. This disease consists, as in the infant, in the mucous membrane of the nasal passages of the throat, of the larynx, windpipe, and bronchial tubes, assuming that sort of inflammation which terminates in the effusion of fluid, more or less loaded with albuminous matter. The researches of Cheyne in this country threw so much light on the seat of this inflammation, on its nature, and on the mode in which it proves fatal, that little that is actually new can be said to have been added by the subsequent labours of Chomel, Bretonneau, and other foreign authors. The most original part of the memoir of M. Louis is a comparative view of croup in the infant and in the adult; and of this, perhaps, the most interesting part is that which treats of the comparative effect of the membranous exudation on the air-passages at these several ages. Authors have invariably remarked the rapidity with which this disease proceeds to its fatal termination in early life; and instances are recorded in which death has taken place in fourteen hours from the invasion of the complaint. M. Louis knows no example of such precipitate destruction in the adult; and he is inclined to think the progress of the disease is always slower.

Though the croupy exudation contracts the air-passages to a greater degree in infancy than in adult age, yet this contraction is seldom adequate to form a mechanical obstacle to the transmission of the air; and as death may take place in infants after the expulsion of the false membranes, the idea of this obstruction to the access of the air is not applicable to all the cases. The same conclusion is still more forcibly illustrated by the observations of M. Louis, in the case of adults, in whom death takes place, though the larynx is actually sufficiently capacious. The notion of spasm of the glottis, advanced by some authors to explain the sudden death of croup, appears to the author quite as capable of explaining the fatal event in the adult as in the infant. These spasmodic symptoms take place only when the larynx is covered with the membranous exudation; and this conclusion, he thinks, receives confirmation from the necrological appearances.

The difficulty which the author here encounters might have been diminished, if not surmounted, by reflecting on the points which Dr Cheyne established on the pathology of croup, and of which, there is some reason to think, he is either unaware or forgetful. The general result of the researches of this physician was,—that the action of croup is not confined, as was thought by Home, Cullen, and others, to the larynx and trachea, but was common to more or less of the bronchial membrane; that its tendency was to cause the formation of a great quantity of puriform mucus, containing a large proportion of albuminous mat-

ter ; and that it was by the obstruction thus occasioned in the bronchial tubes, not by that of the windpipe, or even of the larynx, that death was effected. Though these conclusions are not stated in express terms by the ingenious author himself, yet they are virtually contained in his dissections and pathological inferences ; and M. Louis will find, in the essay of Dr Cheyne, abundant matter for illustrating the subject discussed by himself.

Of the fifth memoir, on *Pericarditis*, the principal object is to ascertain the symptoms attendant on this disease,—its duration and progress,—its mode of termination,—its frequency,—and its causes.

On the first point, the general result is,—that pericardial inflammation is characterized by pain at the precordial region, extending sometimes to the back and to the epigastrium, coming on suddenly with more or less oppression, and in certain cases, but in no definite proportion, with palpitations ; by irregular and intermitting pulse ; and principally by the want of sound in the precordial region, the rest of the left side of the chest being sonorous. To these symptoms are sometimes added syncope, and more or less frequently infiltration of the extremities, which is, perhaps, chiefly when the progress of the complaint is slow. In consequence, however, of the frequency of cases of thoracic disease, in which pericardial pain, irregular pulse, and want of sound in the precordial region are observed, it is only in half the cases that pericardial inflammation can be recognized ; and in several of these, in which it is uncomplicated with peripneumony or pleurisy, its diagnosis is comparatively easy. The sequel of the memoir is good, but does not easily admit of abridgement, and does not present topics of that interest which would justify a full abstract.

The subject of the sixth memoir is one of very great importance ; the consideration of those cases of *malformed heart which consist in the communication of the cavities of the right and left side*, and the consequent commixture, to a greater or less degree, of the respired and unrespired blood. With the view of making a clear and correct arrangement, and of drawing useful general conclusions, to two cases which fell under his own observation in the wards of La Charité, M. Louis has added the principal circumstances of eighteen examples of malformation, collected by various authors from different sources.

The whole number of cases divides itself into five sections, according as the mode in which the cavities communicate is simple or complicated. The first section consists of those cases in which the *foramen ovale* continues open ; the second, those in which the septum of the ventricles is perforated ; in the

third are placed the cases in which one or other of these lesions is combined with a pervious *ductus arteriosus* ; the fourth section contains the cases in which the *foramen ovale* is open, and the ventricular septum is at the same time perforated ; and to the fifth are referred all those instances in which perforations, of the nature here mentioned, are combined with other malformations of the heart. On each of these sorts of organic defect a few general remarks will suffice.

Foramen ovale open.—A degree of patency, as it is occasionally named, in the situation of the *foramen ovale* is very common ; and in a very large proportion of subjects, an oblique or valvular opening may be detected by careful examination of the auricular septum. Such an opening, however, is not sufficient to establish any communication between the right and left auricles ; and it may continue to the end of a long life without causing any inconvenience or uneasiness sufficient to constitute disease. It is only when it assumes the form of a direct aperture in the septum that it deserves attention as a serious organic lesion. In the cases collected by M. Louis, the size of the opening varied from 2 to 12 or 15 lines, and was always proportioned to the dilatation of the auricles. Its shape was more or less round, and its margin was smooth, sometimes whitish, and even fibrinous or tendinous.

The *perforation* of the ventricular septum is not always confined to the same spot like that of the auricular. It may exist in the middle, but was most generally found about the base of the septum. The size of the opening varied from 2 lines to about 1 inch. Its margin was round, variable in thickness, smooth and fibrous. The surrounding parts, however, afforded no trace of lesion ; from which M. Louis thinks it may be inferred, that the opening, like that in the auricles, was congenital, and not the effect of accident,

The same view of the origin of these lesions M. Louis thinks is applicable to the other forms of malformation, even the contraction of the pulmonary artery. It might appear an unnecessary waste of argument to attempt to prove that this is the case, did it not appear that certain circumstances might give probability to the notion, that the communications thus effected were the result of another cause. The open *foramen ovale*, for example, which is always accompanied with more or less enlargement of the auricle, might, without violation of probability, be supposed to be a consequence of this enlargement. Perforation of the septum in like manner, which, in a good proportion of cases, is attended with hypertrophy of the right ventricle, and contraction of the pulmonary artery, might be supposed to be the result of long-continued forcible distension of

this cavity. But while it is easy to see that contraction of the pulmonary artery may, if congenital, contribute materially to prevent the *foramen ovale* from being closed, by reason of the resistance which it affords to the passage of the blood from the auricle to the ventricle;—it cannot be denied that the number of cases, in which the perforated septum is associated with a contracted pulmonary artery and hypertrophied ventricle, is too small to justify the idea, that this malformation is also the result of a similar obstruction.

The point of most importance in the pathological history of this lesion is its effect on the circulation. Since William Hunter and his friend Dr Pulteney first directed the attention of physicians to this subject, it has been very generally imagined, that when a direct communication between the right and left sides of the heart exists, the unrespired blood is necessarily so freely mixed with the small quantity which has been respired, that the object of respiration is very much or entirely defeated; that the blue disease must be produced; and that the individual is slowly wasted by deficient formation of arterial blood, or is suddenly destroyed in one of many fits of difficult breathing. The justice of this conclusion was questioned by Bichat, who maintained, that in the case of open *foramen ovale* at least, no communication of the two sorts of blood took place, not because the opening was not sufficiently ample, but because after birth the current of the blood was impelled in a different direction; and since the two auricles contract at the same moment, the blood, which is impelled by the *musculi pectinati* towards the auriculo-ventricular opening, tends to press the two valvular folds of the opening firmly against each other. No rational doubt can be entertained by any one who is familiar with the anatomical configuration of the auricles, the position of the foramen, or its *vestigium* in relation to the muscular apparatus, and to the openings of the auricle and ventricle, that this opinion is within certain limits well-founded; and it explains happily enough those examples of open foramen, which have been quoted as not being attended with symptoms of disordered circulation during life. Whether it is also applicable to the case of perforated septum will perhaps appear in the sequel.

Without adverting to the train of argument pursued by Bichat, M. Louis comes to the same conclusion; and contends, that, so long as equality of thickness and action continues between the opposite cavities, and their orifices are free and unobstructed, no mixture of the arterial and venous blood can take place, *however large* the opening of the *foramen ovale* may be. Even in the case of inequality of thickness and action, he admits no mixture, so long as the orifices are unobstructed; for, as

the pressure ought to be every where equal, he argues, the blood will escape only where it finds least resistance,—that is, by the auriculo-ventricular opening. From this it follows, that it is only when, to inequality of thickness and action, is conjoined more or less contraction of the discharging orifice, that the blood of the right can be mingled with that of the left side of the heart.

From these facts and arguments, it results, that the circumstances necessary to the mixture of the arterial with the venous blood, at the moment of contraction of the auricles, in the case of open *foramen ovale*, is either a diminished or contracted state of the auriculo-ventricular aperture, or a sensible diminution of the capacity of the ventricle, in consequence of its hypertrophy. And in like manner, in the case of perforation of the ventricular septum, mixture can only take place when the orifices of the arteries are singly or conjointly more or less contracted.

It does not appear, however, that the blue colour of the surface, so often remarked in persons labouring under this malformation, depends solely and entirely on the commixture of the venous and arterial blood. This change of colour, M. Louis remarks, is never found to be in proportion to the freedom of the communication. In one of the cases, though the *foramen ovale* was not larger than a lentil, the blue colour of the face was greatly more intense than in other cases, in which the width of the communication, combined with contraction of the pulmonary artery, ought to have contributed powerfully to the commixture of the blood of the right and left sides of the heart. In other instances, in which this communication must have been congenital, the blue tint did not appear till some years after birth. Thus, in a case given from M. Ribes, in which the aorta was found issuing from the right ventricle, the blue colour, with the other symptoms of diseased heart, did not appear till the age of three years. From facts such as these, therefore, and from the circumstance of the foetus, in which all the blood is of the modena tint, not being blue, M. Louis infers, that this appearance is not the effect of the commixture of the arterial and venous blood; and he agrees with Morgagni in ascribing it to obstruction in the trunk of the pulmonary artery.

The progress of this disease varies. It is in general attended by paroxysms of suffocation and fainting conjoined; but the frequency and the severity of these paroxysms are so variable, that nothing can be predicated with certainty. The duration of life is proportioned, neither to that of the symptoms, nor to the supposed unnatural state of the blood. In two of the cases in which the *ductus arteriosus* continued open, death took place

in the 29th and in the 40th years respectively ; and a woman, in whose body the foramen ovale was found open, the right auriculo-ventricular aperture contracted, the ventricle enlarged and thickened, and the orifice of the pulmonary artery shut up by a horizontal but perforated membrane, had died at the age of 57, not of the effects of these lesions, but of pulpy disorganization of the brain.

The following general results are given :—

1. The communication of the right and left cavities of the heart may take place in several modes ; but the most ordinary is by means of the *foramen ovale* or perforation of the *septum* of the ventricles.

2. It was congenital in the cases here recorded ; and its origin appears to be invariably the same.

3. It coincides in more than half the cases with evident contraction of the pulmonary artery, also congenital.

4. It is invariably accompanied with the dilatation of one or several of the cavities of the heart, generally those of the right side. They are also hypertrophied, which is opposite to what is generally remarked in diseases of the heart.

5. The effect of this communication is more or less mixture of the red and the modena blood.

6. This mixture takes place in all cases at the entrance of the blood into the communicating cavities.

7. It also takes place at its escape from these cavities, when the orifice by which it escapes, is more or less contracted.

8. The blue colour of the skin is rarely universal. It is observed on the face in some instances only during the last week of existence, or it may not be observed during life.

9. It is to be ascribed to some obstacle to the circulation of the blood in the veins.

10. The communication between the cavities of the heart ; the mixture of the two sorts of blood ; and their passage from the right to the left cavities, take place in general, long before the health appears impaired.

11. The symptoms assigned to this communication, viz. the blue tint, the swooning fits, the sensibility to cold, and the sense of suffocation, are simply an aggravated form of those which are generally observed in aneurism of the heart, and are often enough entirely absent.

12. The only symptom which indicates positively the communication now mentioned, is suffocation, more or less violent, returning in very frequent periodical accessions, accompanied or followed by swooning, with or without blueness of the surface, and induced by the slightest causes.

13. The mixture of the red and the modena blood, even to a

considerable degree, at least apparently, is incompatible neither with life moderately prolonged, nor with the developement of the intellectual faculties.

14. It has no sensible or known influence on the progress of intercurrent diseases.

In concluding the foregoing account, it may not be improper to remark, that M. Louis has mentioned two only of the cases recorded by English observers; and that, though he was anticipated both in his arrangement of the subject, and likewise in the most important of his results by Mr Allan Burns in 1809, and by Dr Farre in 1814, no notice whatever is taken of their works. Meckel is not more fortunate; for his name, which is almost inseparably connected with the subject of malformations of the heart, is not once mentioned throughout the whole memoir.

The length to which the examination of these six memoirs has already extended renders it impossible to proceed with the other five, which complete the volume. Though it is necessary, therefore, to make rather an abrupt pause in this place, the consideration of the succeeding essays shall be resumed at some future opportunity.

ART. II.—*Sur l'emploi du Chlorure de Chaux, &c.—Observations on the Causes of Ulcers, the Effect of Chloride of Lime in promoting their Cicatrization, and the Treatment of Varicose Ulcers by Excision of the Diseased Veins, being an Account of the Doctrine and Practice of M. Lisfranc, Surgeon-in-Chief of the Hospital of La Pitié at Paris.* By M. Michel Klimatis. (*Revue Médicale, Novembre, 1826.*)

IT is seldom that the attention of the practitioner is forcibly directed to the common surgical disorders, and still more seldom are they minutely studied by those whose eminence has supplied them with the means of conducting the necessary investigations on a large scale. The object of the following remarks,—the causes and treatment of ulcers,—may appear at first sight one of the most threadbare in surgery. Yet the last good publications on it are not of very recent date. Nor have they by any means exhausted it; for we have only to look to the results of hospital practice throughout the kingdom, in order to be satisfied that the surgery of ulcers is still in all likelihood susceptible of receiving essential improvements. The observations of M. Lisfranc, therefore, appear in good time; and coming from so distinguished a surgeon, and so zealous

and cautious an inquirer, they will be read with interest, even although they have not yet been published in a very regular or detailed form.

His remarks are confined to ulcers which have a local origin, that is, which are independent of constitutional taints, original or acquired. Such ulcers, according to the nomenclature of our countryman Dr Thomson, are known in Britain by the name of *simple* ulcers, and this term has also been chosen by M. Lisfranc. They are best exemplified as they appear on the leg, which it is well known is by far their most common seat. Their situation at so great a distance from the centre of the circulating system, led surgeons of the last century to imagine, that their fundamental cause was a local atony or weakness, and this notion was kept up by their occurring more frequently on the left than on the right leg. It is some time since the inquiries of Sir Everard Home and Mr Brodie drew the attention of British surgeons to the important part which varix of the cutaneous veins performs in producing and maintaining certain kinds of simple ulcers; and although the opinion is nowhere, to our knowledge, laid down in books, we believe it is now not an uncommon idea among practitioners in this country, that ulcers often depend for their origin and obstinacy on the structure of the saphena veins, even when not decidedly varicose. M. Lisfranc, however, ascribes to these vessels a still more extensive agency; for he believes that the natural structure of the saphena veins, when, as happens in many people, there is no valve between the knee and ankle, constitutes a sufficient predisposing cause for the developement of simple ulcers. "The want of valves, which perform so important a part in the venous circulation, becomes in reality an obstacle to the ascent of the blood; the blood obeys to an undue extent the law of gravitation, and consequently it stagnates in the solid tissues, particularly near the surface of the limb. Thus arrested and accumulated, it becomes an irritant, acts in the manner of foreign bodies, overexcites the part mechanically, or in some more obscure way, and presently gives rise to an inflammatory action, which causes more or less rapid disorganization, and, in a word, the establishment of an ulcer." Such, in particular, he supposes to be the manner in which are produced the ulcers that rapidly follow spontaneous inflammation. The chief facts referred to in support of his opinion are the prevailing seat of the ulcers, namely in the interspace between the points between which alone valves are often wanting,—their rarity both above and even below that interspace,—the effect of the horizontal posture in the treatment of them,—the effect of tight-lacing or bandaging in curing them, and preventing their return,—and the radical cure effected by excision of the vein. Before these facts, however, can be allow-

ed the force which M. Lisfranc attaches to them, it must be shown that the ulcers in question are not accompanied also with some degree of varix of the vein, as well as with the want of valves. And we may add, that a certain degree of varix, sufficient perhaps to promote the formation of ulcers, may exist without the surgeon's attention being drawn to it, and without its being discoverable, except by a careful examination.

To account for the more frequent occurrence of ulcers on the left than on the right leg, M. Lisfranc has recourse to a different explanation. The common opinion, which imputes it to the same defects in original conformation that render the movements of the left extremities less powerful and adroit than those of the right, he considers to be contradicted by the frequency of ulcers on the left legs of those who are left-handed; and he thinks a truer explanation may be found in the pressure produced by the contents of the sigmoid flexure of the colon upon the left iliac veins,—a cause which has already been referred to by surgeons as accounting for diseases being more common in the left than in the right testicle. There is much plausibility in this idea.

These remarks on the causes of simple ulcers are followed by some observations on their pathology. When they arise spontaneously, they always, according to M. Lisfranc, commence with the formation of a *phlyctæna* in the middle of the inflamed skin; and when this bursts, a reddish or brownish fluid is discharged, which, if carefully examined when first secreted, is found to be mixed with the *detritus* of disorganized skin. At the same time a soft eschar appears under the fluid, and may be traced gradually extending itself over the neighbouring parts. The process is therefore considered by him, as in the first instance, a variety of gangrene.

When the ulcer is recent, the parts beneath and around it are simply in a state of inflammation. But when the inflammation has continued for a long time, in consequence of the ulcer, its cause, continuing,—the skin and cellular tissue become indurated, and acquire a lardy appearance to the extent of two or three lines beyond the ulceration. And should the disease last for several years, as sometimes happens, the skin, the subcutaneous and intermuscular cellular tissue, as well as the muscles themselves, undergo the same change, occasionally to the extent of two or three inches around the ulcer. Nor does the degeneration of tissue cease even there. At a greater distance the skin has a yellow colour, and tubercles or indurations may be felt here and there beneath it; at a still greater distance the tissues have a pale yellow colour without induration; and beyond all, the tissues are affected with œdema. When such ulcers are in progress towards healing, the altered textures pass

through a succession of changes similar to the progressive differences remarked at different distances around the ulcers; but even after the cure is complete, the skin, wherever it was indurated, has a blanched appearance, as if imperfectly nourished.

Under the head of the Treatment of simple ulcers, M. Lisfranc first takes notice of the idea, still very prevalent, that old ulcers cannot be healed without the risk of being followed by inflammatory disorders of some of the viscera, and more particularly of the viscera of the abdomen. This idea he conceives to have arisen from the fact, that a disordered state of the digestive organs often accompanies old ulcers; in which case the ulcers certainly cannot be healed without the risk, approaching almost to a certainty, of the patient being attacked with one of the internal *Phlegmasiæ*. But he does not consider that the healing up of an old ulcer is attended with any such danger, when the digestive organs are at the time in a state of health. To prevent the danger in question, therefore, he recommends the surgeon, as Mr Abernethy had done long before, never to begin the healing of an ulcer without searching carefully for latent disorders of the chylopoetic viscera; and even when no suspicious symptom can be detected, he thinks it right to stimulate, from time to time, the intestines, kidneys, and skin, by the ordinary remedies. The ulcer ought never to be brought to the point of cicatrization so long as any visceral disorder exists; and in cases in which a disorder of the kind has been removed, he advises the old practice of keeping an issue open for some time in both arms or thighs.

Next follow a few short and general observations on the various plans at present pursued in the healing of ulcers. He concludes his abstract by observing, what every hospital surgeon must be able to confirm, that under the most skilful use of these means many old indolent ulcers are healed up chiefly by approximation of the surrounding healthy skin, and that the cicatrix is consequently stretched tightly over the subjacent parts, and is apt to give way under slight external force, or even under the action of the muscles beneath. Against this inconvenience M. Lisfranc thinks he has discovered an effectual remedy in the *Chloride of Lime*, which, according to the result of many experiments at the hospital of La Pitié, possesses the singular property of inducing cicatrization without contraction of the margin of the ulcer.

He found, that when the ulcer was in an irritable or inflamed state, it was rendered still more so by the chloride of lime,—that, so long as the parts around the ulcer were in an indurated state, a cicatrization was never induced by it, even although the surface had granulated,—and that, when the ulcer was in a

healthy state, namely, when the indurated margin was softened down, and granulations were formed, a cicatrix might be often formed by it at any stage in the filling up of the cavity of the ulcer. Hence, before using this application, it became necessary to resort to a variety of preparatory measures. To subdue the inflammatory state of an ulcer, he has found no treatment more effectual than the abstraction of blood by leeches applied to its surface. The induration of the surrounding tissues he has also sometimes removed by the same means employed every two or three days. If repeated leeching failed, crucial incisions practised in the way recommended by Paré had sometimes the desired effect. And when that plan also failed, he removed the altered tissues by cauterization, which was effected sometimes with a burning glass, sometimes with the canterizing iron, sometimes with the protonitrate of mercury. These preliminary steps having been taken according to circumstances, and healthy granulations having been established, it was requisite to attend to another preliminary point, namely, that the granulations were neither above nor below the level of the surrounding skin. For he found, that in both circumstances the chloride of lime has a tendency to effect cicatrization, and consequently to leave a prominence or a depression on the seat of the ulcer.

In the ordinary way of healing ulcers the cicatrization proceeds slowly from their circumference to their centre by the secretion and consolidation of coagulable lymph on their edges. But, according to M. Lisfranc, when an ulcer is washed with a solution of chloride of lime, the plastic matter which forms the matrix of the cicatrix, in other words the coagulable lymph, is poured out over the entire surface at once, or at least in rapid succession from the circumference to the centre; and the whole ulcer is covered before its edges have time to contract. Besides that advantage, the cicatrix possesses much greater elasticity than those formed in the usual way, and sometimes, indeed, it is hardly less elastic than the skin itself. Farther, it is also much stronger: In many cases, in which the common cicatrix gave way under the action of the muscles soon after its formation, the cicatrix formed by the chloride of lime resisted effectually. In a few instances, however, in which the cicatrix was of unusually great size, it did yield; but in some of these, M. Lisfranc found it was subsequently made of sufficient strength by keeping the limb at rest for two or three months,—a precaution which would save many old ulcers from breaking out again so easily as they generally do.

The strength of the solution he uses is regulated by Gay-Lussac's chlorometer. In general he commences with a solution of three degrees of strength; and sometimes he has in-

creased it to ten degrees. The chloride of soda has the same effect as the chloride of lime.

The treatment by the chlorides of lime or soda has no advantage over any other in cicatrizing ulcers which depend on *Varicose Veins*. In these cases M. Lisfranc has revived the practice mentioned by Celsus, (vii. 31.) of removing the varix by excision of the vein.

He introduces this interesting subject with a short historical notice of the various operations proposed in ancient and modern times for the removal of varix. In this sketch he has most unaccountably omitted the name of Mr Brodie as the first proposer of the method by Incision, now adopted in this country as being at least the safest of all the operations hitherto tried; and has ascribed its discovery to his countryman Beclard. The operation suggested by M. Lisfranc differs from that of Mr Brodie in requiring a portion of the vein to be removed altogether; and it differs both from that method, and from the ancient operation of excision, in being applied not to the varicose part of the vessel, but to a healthy part higher up. His object in removing a portion of the vein is to supersede the use of the ligature, which has been abandoned as too dangerous by every recent British and French surgeon. His object in operating on a healthy part of the vessel is to avoid what he has been led to consider one of the principal causes of the inflammation of the vein, which too often succeeds every variety of operation for varix. He says, that in many cases varix is accompanied with *hypertrophia* of the coats of the vessel, and with induration of the cellular tissue around; in consequence of which the divided extremities of the vein remain gaping, and the air rushes in at the upper orifice. To this accident he imputes the inflammation of the vein, which sometimes follows the simple division of the diseased vessel, and which is so violent that he has sometimes known the patient die within twenty-four hours after the operation. In all cases, therefore, he prefers operating on a part of the vessel above the seat of the disease, and consequently in general immediately above the knee-joint.

The operation is performed in the following manner: The patient being laid on his back and the affected limb turned outwards, the surgeon places himself on the outside of it, bends the leg a little on the thigh, and directs an assistant to compress the vein higher up by grasping the whole limb with both hands. An incision about an inch and a half long is then to be made with a convex-edged scalpel, along the course of the vein, through the skin and cellular tissue; the vein is next raised up by a director thrust under it; and it is divided with the scissors at each angle of the incision. The blood is allow-

ed to flow freely for some time ; and if it does not issue freely, an equivalent blood-letting is practised at the arm. A compress is then applied on the vein,—never where it is wounded,—but two or three inches higher up, and also at the same distance below, if the lower orifice should bleed as well as the upper. The wound is healed by the first intention, and a few hours afterwards the compresses are removed, in order to avoid as much as possible every risk of inflammation. Should *Phlebitis* appear notwithstanding all these precautions, a great number of leeches are immediately applied along the course of the vein from the superior angle of the wound upwards ; and after they have fallen off an emollient poultice sprinkled with laudanum is laid over the thigh. At the same time the inflammation of the part of the vein and limb lower down than the incision is allowed to run its course, on the supposition that it promotes the cure of the upper part by acting as a derivative. Since adopting the foregoing operation and mode of treating the phlebitis which occasionally supervenes, M. Lisfranc has cured many cases of varix, and has not lost a single patient even when inflammation did make its appearance. Sometimes a double operation was requisite on account of both saphena veins being varicose.

When the operation is undertaken for the cure of a varicose ulcer, the ulcer in a short time, sometimes even in thirty hours, begins to assume a different appearance. Its peculiar dark-red or violet colour is succeeded by a paler hue, the discharge becomes less abundant, and assumes the characters of healthy pus ; and under the treatment formerly laid down a cicatrix is rapidly formed.

We shall close this sketch with an abstract of the only case of excision of the saphena vein which has hitherto been published by M. Lisfranc. A man forty years of age, and of a lymphatico-sanguine temperament, was admitted into the hospital of La Pitié on account of an ulcer on the left leg as large as the palm of the hand, and accompanied with complicated varix and induration of the veins. All the ordinary methods of treatment had been tried without success. An inch and a half of the internal saphena vein was therefore cut out in the manner mentioned above. The upper orifice only discharged blood, which, after it had flowed some time, was checked by pressure applied at some distance above the wound. The wound was dressed so as to heal by the first intention, and the patient was kept on low diet. Next day the ulcer had become pale, and there was not any pain felt in the wound. On the second day the upper end of the vein being painful and hard, twenty-five leeches were applied along its course, and afterwards an emollient poultice sprinkled with laudanum. On the third day the leeches were applied again, as the phlebitis remained in the

same state; and on the fourth, no amendment having taken place, they were repeated again. On the fifth the inflammation had entirely disappeared. The emollient poultices and low diet, however, were continued. The ulcer had assumed a vermilion colour, the suppuration was healthy, and cicatrization had commenced. The patient was dismissed cured two months after his admission, and a few days after the cicatrix was completed. But the cicatrix had been formed upon an indurated vein, and gave way a few days after he resumed his work. Being again admitted into the hospital, the limb was kept constantly at rest; leeches were applied every other day on the surface of the callosity, and poultices were kept on during the intervals. The induration then gradually disappeared, and with the aid of the chloride of lime, a cicatrix was formed in twenty-five days after his return. The limb was nevertheless kept at rest six weeks longer; after which he was again dismissed. M. Lisfranc has frequently heard of him since; and the cure continues permanent.

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- ART. III.—1. *A Treatise on a Modified Application of Moxa in the Treatment of Stiff and Contracted Joints, and also in Chronic Rheumatism, Rheumatic Gout, Lumbago, Sciatica, Indolent Tumours, &c. &c.* Second Edition. By JAMES BOYLE, Esq. Surgeon of the Middlesex Infirmary, author of a Treatise on the Cholera of India, &c. &c. London, 1826.
2. *A Physiological Enquiry respecting the Action of Moxa, and its utility in Inveterate Cases of Sciatica, Lumbago, Paraplegia, Epilepsy, and some other painful paralytic and spasmodic Diseases of the Nerves and Muscles.* By WILLIAM WALLACE, M. R. I. A. &c. &c. Surgeon to the Charitable Infirmary of Dublin, &c. &c. Dublin, 1827.

WHEN a new or unknown remedy is introduced to public notice, or the employment of an old one is revived, it is not merely requisite to describe the benefits resulting from its application. The theory of its operation should, if possible, be understood; the circumstances which indicate its employment should be explained; and it is of the utmost importance to remove whatever prejudices may oppose its just claims to the confidence of the profession.

Moxa, which has been long known as a powerful and much used remedy among the Chinese, Japanese, and other oriental nations, but was till lately little known, and less practised in Europe, stands very much in this predicament. Though introduced in 1821 to the attention of his countrymen, by the ac-

tive recommendation, and supported by the powerful influence of Baron Larrey, its employment is very much confined to the practice of the hospitals of Paris. In this country the use of *Moxa* is still more limited; and not even in the principal cities can it be said to be a remedy in general, or even in moderate employment. There is little reason to suppose that this results from the small number of cases which require its application. It is very likely that the eastern nations use it too much and too frequently; and it is not improbable that our foreign neighbours are not particularly discriminating in the cases to which they apply it. But cases in which it is admissible or requisite, cannot vary so much in frequency as to account for the rarity of the use of *Moxa* in this country. Its merits may not be sufficiently known; or, what will have a similar effect, it may not have sufficiently obtained the confidence of practitioners. If its mode of operation be not understood, many who would willingly give it a trial, would decline the administration of an agent, the action of which they did not comprehend, and consequently, could not rationally explain. In some instances, dislike to innovation, and to the employment of remedies which were unknown or unheeded in the days of their instruction, may tend to operate against the use of the heroic *Moxa*. Is it inconsistent with the history of science, and the nature of the human mind, to add, that not a few, from feelings of indifference and indolence, are reluctant to employ a remedy, the properties of which are still to be learnt by many experimental essays, and not a little careful observation?

To obviate these, and similar objections against the use of *Moxa*, is the chief purpose of the two publications here introduced to the attention of the English reader. Though both authors digress somewhat, and wander into discussions a little remote from the line of inquiry; yet with both, the principal object may be said to be,—to investigate and explain the physiological action of *Moxa*, to describe the several methods best calculated for obtaining its remedial effects, and to define the cases, or sort of cases in which it is indicated, and is most likely to prove beneficial.

It may perhaps be questioned, whether either author has succeeded in his undertaking; and perhaps, neither in the work of Mr Boyle, nor in that of Mr Wallace, will the scrupulous inquirer find all the information and all the evidence which he wishes to obtain. But if neither of the two works is the best that might have been expected, it may be answered, they are the best that are at present to be found in the English language. Though perfection is rarely attainable at first efforts, it is always desirable to use such information as can be obtained; and the thanks of the profession are justly due to Mr Boyle and to Mr Wallace, even for the efforts which they have made to ascer-

tain the properties, and explain the merits of an agent which promises to be an active remedy in the practice of the healing art. In the remarks which are now to follow on their works the order mentioned above shall be observed, without reference to those digressive excursions in which the authors have indulged.

No one needs to be told, that the term *Moxa* is of oriental extraction ; and that whatever be the sense in which it was originally used, at present it is understood to signify the slow application of heat to the surface of the body, by means of some vegetable substance which burns so gradually as to produce, according to the intention of the operator, any given effect, from simple redness, or vesication, to a complete eschar.

The substances which have been employed for this purpose, vary at different times, and among different nations, according to the facility with which they may be obtained, or the caprice of the individuals who use it. The Chinese and Japanese use the down of a species of mugwort, (*Artemisia*,) obtained by rubbing the dried leaves between the fingers. The Hindoos are said to use the spongy pith of a species of rush ; the Persians, goats' dung, dried and rolled into cylinders ; and the Armenians, the agaric of the oak. Among the Ostiaks and Laplanders, the agaric of the birch, or birch-wood itself when rotten and phosphorescent, is said to be the substance principally employed. The Egyptians, it is supposed, were the first who had recourse to cotton ; and this substance, under various modifications, has been more or less generally used for the same purpose in Europe.

In modern times, since the subject has attracted the systematic attention of the profession, the number and variety of substances has been somewhat diminished ; and the object has been to use the substance most conveniently obtained, to produce the wished for result. Sydenham is said to have employed flax. Baron Percy prefers the pith of the sun-flower (*Helianthus annuus*) ; and when this cannot be got, he substitutes cotton impregnated with a saturated solution of nitrate of potassa. Baron Larrey uses a cylinder of carded cotton, unimpregnated and surrounded with linen ; and in imitation of this, the general practice, in the Parisian hospitals, is to use calico rolled so tightly as to form a firm cylinder, which is held to the part by a pair of dressing forceps. Mr Wallace conceives the best cylinders are formed by immersing charpée or fine linen in a solution of chlorate of potass in the proportion of one drachm of the salt to four ounces of distilled water, and by rolling it up, when thoroughly dried, so firmly, that it may afford a perfectly smooth clean section, when divided with a very sharp knife.

On the mode in which moxa operates, the opinions have been

various, and not well-defined. By those who were desirous to simplify their views, its action was conceived to be quite analogous to that of the cautery, actual or potential. To this opinion, however, the present authors are decidedly hostile; and Mr Boyle especially adduces several cogent arguments to show that the peculiarity of its action is destroyed when it is applied as a mere cautery. Baron Larrey, in admitting that its operation was very different from that of the actual cautery, had advanced the problematical notion that moxa appears to communicate with a relative mass of caloric an active volatile principle derived from the burning cotton,—by virtue of which it excites a degree of irritation which is sooner or later communicated to the surrounding and deep-seated parts. Overlooking entirely the imaginary notion of the *active volatile principle*, this opinion evidently ascribes to moxa the qualities of counter-irritation, and refers its good to the effect of the discharge; and in certain circumstances this is indeed the case.

Both Mr Boyle and Mr Wallace, however, agree in regarding moxa as a substance which operates in some mode quite different from that of counter irritation. The former author remarks that it has been “successful in removing painful affections of nerves, where no surrounding or secondary irritation was produced, and where, consequently, discharge, or counter-irritation, could have no particular influence.” In his own practice he represents its application to be so mild as not to cause the slightest abrasion of the cuticle, and yet to effect cures in cases in which all the known remedies had failed; and he therefore infers, that counter-irritation or discharge is by no means a necessary attendant on its general employment. So far, indeed, does Mr Boyle carry this view of irritation and discharge not being essential, that, in the majority of cases, he regards them as positively injurious; and he contends that the formation of an eschar or slough is quite inconsistent with the peculiar and beneficial effects of the remedy. The reasoning of Mr Wallace is very similar. Moxa, when applied properly, and in appropriate cases, he contends, produces neither inflammation nor discharge; and if it excite much inflammation, he regards it as a proof that it is either not suited to the case, or that it has been incorrectly applied. On many occasions no discharge takes place; but when it does, the good effects of the moxa appear long before the discharge is established.

Having shown, or attempted to show, what is not the operation of the moxa, it remains to examine what is; and on this point it is by no means so easy to come to a satisfactory conclusion. Mr Boyle, indeed, though he contends that the benefit of moxa is to be ascribed neither to counter-irritation, nor to

discharge, nowhere gives any direct or positive explanation of the mode in which it proves beneficial, unless that it acts as a stimulant to the absorbents ; and the only proof which he adduces of this opinion is, that chronic swellings, after two or three applications, scarcely causing discoloration of the skin, though previously obstinate, begin to subside rapidly. Mr Wallace, on the other hand, gives a view of its action, which, though certainly more explicit and palpable, labours under the disadvantage of not being susceptible of very direct demonstration. The operation of moxa on the human body depends, he maintains, on the heat or caloric disengaged. The effect of heat again is to excite the capillary vessels of a part to contraction by augmenting their tonicity, and, consequently, to diminish their calibre, and increase the rapidity of their circulation (p. 32 and 36) ; and he thence infers, that moxa acts like a stimulant, astringent, and local tonic, which powerfully excites the capillaries to forcible contraction and a rapid circulation of the blood conveyed, and which, at the same time, excites remarkably the absorbent vessels. (46.)

It is reasonable to expect that Mr Wallace should support this theory with facts and arguments of competent strength and cogency ; and it is but justice to admit that he has not been altogether unsuccessful in appealing to facts which are certainly not very dissimilar to the operation of moxa. Thus, he quotes from the writings of Drs Hastings and Wilson Philip four experiments in which heat applied in different ways was followed by contraction of the capillaries of the part and increased velocity of the circulation. In a gangrenous ulcer in the leg of a woman the combustion of a moxa cylinder over its surface was followed by evident diminution of the size of the capillaries. In a case of chronic inflammation of the conjunctiva, accompanied with great dilatation of its capillaries, moxa applied over the eye was followed by contraction of the capillaries half an hour after the application ; and in an ulcer on the breast of a child, resulting from a burn, when the granulations, which were large, pale, soft, and much disposed to bleed, could not be kept down by the ordinary means, a single application of the moxa over them reduced them in the course of one night much below the level of the surrounding skin.

These facts must be admitted, if there is credibility in human testimony, as proofs, that one effect at least of moxa is to diminish the calibre of the capillaries of the part. It by no means follows, however, that the sole and invariable operation of moxa is to contract the capillaries. This contraction, according to the showing of Mr Wallace himself, is almost

an immediate and a temporary effect. The physiological observer, however, not satisfied with this conclusion, inquires into the duration of the capillary contraction, and he finds that it is not only not permanent, but that it is followed by another state which, though not easily described, certainly cannot be said to consist in contraction only. In short, notwithstanding all the arguments with which Mr Boyle and Mr Wallace have laboured to overthrow the theory of counter-irritation, it is perhaps still the most convenient and the most consistent with physiological principles that has yet been advanced.

Both authors, perhaps, have allowed themselves to be misled by the idea that discharge is essential to this hypothesis. But it is surely unnecessary to show, that this is quite a mistaken view. Discharge takes place only where the inflammation is so severe as to proceed either to vesication, to suppuration, or ulceration, from the production of an eschar. There are, however, several degrees of inflammation, from the most transient blush up to erythema, more or less permanent, which do not terminate in any sensible discharge, and which may, nevertheless, be sufficiently active on the surface to counteract a moderate or deeper action in the substance of a part. It is farther to be remarked, that the benefit produced by superficial inflammation is in proportion not to the discharge, but to the inflammation or irritative action which results; and this would have perfectly obviated the objections urged by both authors. In short, it is matter of observation, that in the case of eschars produced by the cautery, actual or potential, the benefit results not from the discharge, but from the inflammatory action on which the discharge depends. When this action is over, the benefit ceases; when it is renewed the salutary operation is re-established.

Nor is this view of the operation of moxa confined, as Mr Boyle and Mr Wallace contend, to the case in which the remedy is allowed to produce an eschar. It is equally applicable to every degree and every form in which it may be applied;—that is, at least, where any sensible effect at all is produced. The only difference is this,—that in the milder forms, where it is allowed to produce redness only, or vesication, or slight scorching, the inflammation takes place in the part to which the moxa has been directly applied; whereas, when it is allowed to produce a slough or eschar, the inflammation does not take place in this part, because it is entirely dead, but in the contiguous and subjacent living textures, which are thus excited to action in order to remove as it were the portion of dead and foreign matter. The opinion advanced by Mr Boyle, that in this case the slough forms an obstructing medium, which inter-

rupts the powers of the remedy (p. 27,) and prevents the caloric from reaching the seat of the disease, is exceedingly fanciful, and not very consistent with good physiology or pathology either. The caloric, doubtless, cannot penetrate to the seat of the disease through a dead slough; but is it consistent with the laws which are known to regulate the transmission of caloric, to suppose that it will penetrate any better through the living texture? If Mr Boyle allows physical principles and facts to regulate his conclusions, he will find it difficult to answer in the affirmative. It must be remembered that it is not the caloric, but what the caloric does, that produces the benefit. It is neither the heat which forms the eschar, nor the eschar itself, but the irritative and inflammatory action established at the bottom, and round the edges of the eschar, which is the therapeutic agent. It is needless, however, to dwell longer on this point of the physiological action of the remedy. It is inconsistent with the plan of these remarks to enter into any full investigation of the subject; and it is sufficient, if the reader be informed of the theory adduced by Mr Boyle and Mr Wallace, and be aware of the objections to which it is liable.

The mode in which moxa may be applied, varies according to the convenience of the operator, and the effects which it is intended to produce. Mr Boyle proposes a small brass syringe, the further end of which is provided with a thimble-like apparatus to contain the moxa, and a valve to allow the escape of the air, apparently for the purpose of exciting it when burning. The instruments used by Mr Wallace are a *porte-aiguille*, contrived by himself, or a pair of dressing or artery forceps with a screw, a bit of flat silver wire, and a blowpipe. The moxa cylinder is enclosed in the silver wire formed into a hoop, the ends of which are grasped by the forceps, and secured by tightening the screw.

On the different degrees in which it is desirable to obtain the effects of moxa in individual cases, Mr Wallace has bestowed particular attention; and the result of his experience and reflection will perhaps be most properly communicated in his own language.

“It may be used, so as not to cause any injury of texture; in a greater degree so as to produce vesication; and in a still greater degree an eschar, and the eschar may be either deep or superficial; or, lastly, it may be employed in conjunction with the acupuncture needle. These different modes of using the moxa may be distinguished by the terms, first, second, third, fourth, and fifth forms of application.

“The first form of application will answer when the disease is very superficial. It constitutes the objective cautery of the French writers, and is highly extolled by *Faure* in the *Mémoires de l'Académie*

Royale De Chirurgie, Tome xv. 12mo, as a powerful remedy for the cure of ulcers. It may be serviceable in neuralgia, when the nerve is very superficial; or in affections of the joints, when the synovial membrane is immediately under the integuments, as in the case in the knee and wrist. When used, it should be repeated at least once a day, and applied by holding the moxa in the forceps, as close to the part as the patient can comfortably bear; at the same time it should be moved slowly over the surface, backwards and forwards, until its combustion has terminated.

“I seldom have recourse to the second form of application, because it is not so effectual as the third, and it is more troublesome in its after treatment. It may, however, be usefully employed in *Tic Douloureux*, and to those parts, on which the patient would not wish a cicatrix to be formed. In such cases the moxa is applied by holding it steadily, and as close as possible to the skin, without allowing it to touch it, and until the skin appears white; which appearance is owing to the detachment of the cuticle, and the formation of a blister.

“In a large proportion of cases the superficial eschar will be the best form of application. To produce this eschar, the moxa must be placed on the skin, and allowed to remain on, until the skin appears brown under it; which will, in general, be found to take place, when the combustion of the moxa has extended to the distance of about a line from the skin.

“The deep eschar will be required, when the seat of the disease is far removed from the surface, as in affections of the spinal marrow and of the hip. To form this eschar the moxa must be allowed to remain on, until its combustion is complete; when the part upon which it was seated will be found black, and the surrounding skin slightly red, and wrinkled. In this form of application, it will be sometimes useful to increase the intensity of the heat by the employment of the blowpipe; and when this is thought prudent, the moxa should be, previously to its application, surrounded by a cylinder of card-paper, which will have the effect of directing the current of heat downwards, and prevent its escape laterally.

“When our object is to obtain any of the effects last described, it will be advisable, previously to the application of the moxa, to mark the spot on which we may wish to place it, with a little circle of ink; and in case of using the blowpipe, we should cover the surrounding parts with a piece of paper, having a hole in its centre for the moxa: the paper having been previously wet in a saturated solution of the sulphate of alum, or muriate of soda, and afterwards dried. These solutions diminish so much the combustibility of the paper that it will prevent it from taking fire, in case a spark should be driven off by the blowpipe.

“On several occasions, as for example in paralysis, and some obstinate forms of sciatica, the frequent repetition of the moxa will be required, before the disease is subdued; and the disease is so far removed from the surface, that, to act on it, an eschar must be produced. When the eschars are thrown off ulcers are formed, and where

they are very numerous, they cause considerable irritation to the patient, and the discharge produced by them may be more than is suited to the weak state of his system. In these cases the application of the moxa, in conjunction with the needle, will be found a most excellent mode of using the remedy.

“When the moxa and acupuncture needle are used in conjunction, the following is the mode of operating. I perforate a moxa of a proper size by a needle, of such a length, as will be sufficient to reach to the seat of disease, and at the same time extend so far beyond the surface of the skin, as to keep the moxa about one inch from it, or so far as to secure the texture of the skin from injury. The needle is then introduced as far as the seat of disease, by the assistance of the *Port-aiguille*; and, as soon as it has been introduced, the *Port-aiguille* is removed, the needle being left in the part. The moxa, which had been previously perforated, should be now placed in a state of combustion on that end of the needle which projects beyond the surface of the skin, and allowed to burn round the needle by which it is thus transfixed. The heat disengaged from the moxa is thus communicated to the needle, and thence conveyed to the seat of disease. When the needle has cooled it is removed, and the wound or eschar produced by it is scarcely observable.” Pp. 69-72.

In the present state of professional knowledge regarding moxa, one of the most important points of inquiry is, to ascertain in what cases particularly its use is indicated, and to determine if there be any particular marks by which the surgeon can be guided in anticipating from its application favourable results.

The first class of affections, and perhaps the one most frequently and generally demanding the application of moxa, is that of the joints, and the articular textures in general. It is not, however, to be supposed that the remedy is, in all instances, in every form, and in every stage of these diseases, equally admissible. In the present stage of the inquiry, indeed, little positive information has been collected on the relative influence which moxa possesses over different affections of the joints; and the exact form of disease to which the remedy is most suitable, is still a subject on which accurate facts are wanting. Mr Boyle gives nine cases of diseased knee-joint, eight of which were stiff and contracted, in which, after all other means had been tried in vain, moxa was eminently successful in restoring the motions of the limb. So far as can be learned from perusal of the cases, these instances of contracted joint appear not to have depended on chronic inflammation or ulceration of the articular apparatus, but on that species of contraction which is not unfrequently one of the worst effects of rheumatic inflammation. The same view may be taken of the two cases of hip-joint affection, of that of the wrist, and that of the ankle, likewise sketched by Mr Boyle. So far as is hitherto known, there-

fore, it is in contractions of the joints, dependent on affection of the extra-articular textures, perhaps the aponeurotic sheaths and tendinous fasciæ, that the action of moxa is most certainly useful.

On the use of moxa in affections of the joints, depending on disease of the intra-articular tissues, neither of the works at present under consideration afford any intelligence whatever; and this is perhaps the more to be regretted, because there is reason to think that it is on the due and seasonable application of moxa to such affections that its chief merit and reputation will depend. Since the structure of the joints has been more accurately studied, and since their affections have become better known, the surgeon is more capable of accommodating the means of cure to the nature of the disease requiring treatment. Inflammation of the synovial membrane may be acute, sub-acute, or chronic, but is more frequently acute than otherwise. When this action spreads to the cartilages, or originates in them, it almost invariably terminates in ulceration, a chronic process which, if the patient survives, tends naturally to ankylosis. When the inflammatory action, on the other hand, spreads from the synovial membrane to the capsular ligament, chronic disease of a most serious, obstinate and intractable description ensues; the ligament becomes thickened and indurated; the surrounding cellular tissue is extensively infiltrated with a sero-gelatinous fluid; it afterwards becomes hard and granular; chronic inflammation is established in particular spots, and gives rise to tedious and unhealthy suppuration; sinuous abscesses are formed between the tendons and the ligaments; and if the constitution of the patient is not utterly undermined by the pernicious process, his prolonged existence is embittered by the burden of a swelled, inflexible, and useless joint, and an emaciated but cumbersome limb.

It must doubtless be a matter of some moment to ascertain to which of these affections, if to any of them, and to what stage of them, the employment of moxa is likely to be advantageous. In the acute stage of synovial inflammation, or when the disease is of short continuance, the ordinary antiphlogistic means of local bleeding by leeches and cupping will, in the majority of instances, be amply sufficient to control the urgent symptoms. It is chiefly when the disease is of some standing, and where the pain, instead of being acute, becoming dull, heavy, and deep-seated, denotes the transition of the action to the cartilages on the one hand, or the capsular ligament on the other, that the application of moxa may be expected to be beneficial. When, again, the capsular ligament is chronically inflamed, when the articular cellular tissue is infiltrated with fluid, or when there is reason to

believe that the cartilages are undergoing ulceration, then repeated combustion with moxa, to the degree of inducing an eschar, superficial or deep, according to circumstances, may be expected to afford considerable, certain, and permanent relief. It is of no moment which joint be affected, for the remedy appears equally suited to each and all of them. But in general the superficial eschar seems to do best with the knee, and the deeper seated one with the hip-joint. When the knee-joint, however, has proceeded to that state in which the articular cellular tissue becomes infiltrated, the deep eschar is the most useful form of the application.

The next class of affections in which the use of moxa is indicated, is that of the textures composing the spinal column; the intervertebral fibro-cartilages, the intervertebral articular ligaments, the synovial capsules of the superior and inferior articular processes, the vertebral periosteum, and even the vertebræ themselves. After all that has been recently written on the subject of spinal diseases, information is still wanting on the influence which each texture exerts, and on the peculiarities which spinal diseases derive from the more or less considerable affection of any one of the constituent textures. It cannot be said that there is in nature no foundation for this mode of distinction; for every derangement of the spinal column will be found, on attentive examination, to assume peculiar features, according as the vertebræ, the intervertebral cartilages, the ligaments, or the articular capsules are more or less affected. It cannot be doubted, therefore, that an examination of the comparative influence and curative powers of moxa, in these various forms of disorder, would be an essential service to the practice of surgery. The "inquiry" on this subject, in the work of Mr Boyle, is by no means calculated to supply this defect; for, from whatever cause it arises, his observations are of that general and vague description, that they give no precise or specific information, either on the peculiar forms of spinal disease, or on the appropriation of remedies to these forms. The present, therefore, may be a good opportunity for directing the attention, not only of Mr Boyle, but of those practitioners who have leisure and inclination, to an inquiry which, under the views above suggested, promises some improvement to the art of surgery.

Under the present head also may be mentioned another very untractable disease, in which the application of moxa may be expected to be highly advantageous. The tedious and unhealthy suppuration termed lumbar abscess depends in too many instances on chronic inflammation of the vertebræ, causing caries of these bones, and terminating in suppuration of the contiguous parts. But whether this morbid state of the bones, or suppurative inflammation of the psoas muscle and cellular

tissue, be the pathological cause of lumbar abscess, it is well ascertained that it is a chronic complaint; that its inflammatory stage is over long before the matter presents itself, either in the back, in the groin, or at the anus; and that the most probable mode of opposing the progress of this formidable disease is by the early use, first, of topical bleeding by means of cupping or leeches, or both; and secondly, by the energetic employment of those agents which, by inducing a permanent superficial vascular action, hold out some prospect of counteracting that which is deep-seated. Among these means it cannot be denied that moxa holds a conspicuous place; and as such, it may be employed with well-grounded hopes of affording at least considerable benefit.

The next class of complaints to which the use of moxa promises benefit, are those instances of obstinate chronic rheumatism which depend on chronic inflammation of the aponeurotic coverings and sheaths of the muscles. In lumbago, sciatica, rheumatic gout, and the whole train of those maladies, equally perplexing to the physician and distressing to the patient, the application promises the happiest effects. Both in the work of Mr Boyle and in that of Mr Wallace, especially, the cases of this description, not only relieved, but so far as judgment can be formed from the reports, completely and permanently cured, afford the strongest testimony of the efficacy of the remedy. Several of these cases were of long duration, some for months, and one or two for years; yet in all, the complaints appear to have yielded to one or more applications of the moxa. Though it may not be prudent to trust implicitly to the reported powers of the remedy, and this is said without the slightest doubt of the credibility either of Mr Boyle or of Mr Wallace; yet its actual effects, after the failure of other remedies, prove its remedial powers in such cases to be sufficiently great to entitle it to very considerable confidence.

Analogous in some respects are those painful affections referred to the neurilematic coverings of the nerves, under the names of *tic douloureux*, *neuralgia*, *neurilemitis*, &c. On these subjects Mr Wallace has made some good observations, and collected several cases of some interest, to which the reader may refer, without the fear of being disappointed. The examples of nervous sciatica are particularly instructive, and bear every mark of being carefully observed, and treated with efficiency and success.

It will not do, however, to employ moxa indiscriminately in all complaints whatever. Baron Larrey states, that he uses it in the treatment of consumption; and Mr Boyle speaks as if he thought its application to the extent of producing a deep slough

might be beneficial. This is the very way to injure the real merits, whatever they be, of any remedy, and to make it be represented as a practice either useless or injurious. When consumptive symptoms are known to depend on the presence of tubercles, which are undergoing the softening process, moxa and all such remedies will be not simply harmless, but directly hurtful, by the irritation they produce, without the chance of giving any relief. There is, however, one form, if not two, of pulmonary disease, in which its use is admissible. When consumptive symptoms depend, as in many instances they do, on chronic bronchial inflammation, or on chronic peripneumony, the employment of moxa, after suitable blood-letting, and with the aid of other remedies, will often tend very powerfully to remove the visceral inflammation. True it is, the operation of moxa in such cases presents nothing specific; and it is in all respects similar to that of a caustic issue, or rather of the inflammation succeeding to the eschar produced by the caustic. But the long superficial action maintained by this means is sometimes remarkably successful in abating cough, relieving oppression and tightness in the chest, in diminishing expectoration, and reducing the pulse to the natural standard. This is often remarkably the case in those instances of long-continued pulmonary disease, in which frequent hemorrhage from the lungs is a prevalent symptom.

The foregoing sketch will, it is hoped, enable the surgical practitioner to form some notion of the degree of reliance which he is to place on the virtues of moxa, and to distinguish the circumstances in which it is most likely not to disappoint his hopes. It is quite possible that its merits may be overrated by such zealous advocates as Baron Larrey, Mr Boyle, and Mr Wallace, exactly as they are underrated or suffered to languish in neglect and oblivion by the greater number of the profession in the British dominions. Enough, however, has been done by the authors now mentioned to show that, under proper management and seasonable application, it may be rendered an efficient and available agent of surgical practice. It remains for the practitioner to follow the example now set him; and, by putting the remedy to the test of experiment, in cases similar to those of Mr Boyle and Mr Wallace, either to confirm, to refute, to modify, or to correct the results obtained by these gentlemen. Whatever be the consequence of such inquiry, as to the ultimate fate of the remedy, it cannot be doubted that it can have no other than a beneficial influence on the practice of surgery, and the treatment of surgical diseases, by showing with greater precision than has been hitherto done, where moxa is to be remedial, and where it is not.

ART. IV — *Recherches Nécroscopiques, &c. Necroscopic Inquiries on certain changes which take place after death in the appearance of the Blood-vessels, Lungs, and the Pulmonary and Intestinal Mucous Membrane, in the Healthy State.* By MM. RIGOT of Alfort, and TROUSSEAU of Tours. [Archives Générales de Médecine, Octobre et Novembre 1826.]

IT is singular that amidst the general and minute attention which has of late been paid to morbid anatomy in France, and the daily additions which have in consequence been made to pathological knowledge, few have been induced, by the opportunities which that country affords, to institute a connected inquiry into the nature, causes, and characters of pseudo-morbid appearances. It must be apparent to every one, who has had occasion to consult extensively the pathological writings of the French, that they are in general but imperfectly acquainted with this important subject. Nay, it would be an easy matter to show, that the want of a due knowledge of the existence and characters of pseudo-morbid appearances has been in a great measure the cause of the errors of a prevailing sect of Parisian physicians, and of the popularity which their doctrines have attained to. If in our own country the speculations and tenets of Broussais have met with a colder reception, it may be attributed partly to the sound portion of the practice which he has deduced from them having long formed an essential feature in British physic; but it has evidently been also in part owing to a conviction that, in a large proportion of instances, the anatomical derangements referred to as the proof and foundation of the Broussaïc doctrines, are really, as MM. Rigot and Trousseau have shown, the consequences of changes effected in the healthy tissues after death.

Even in this country, however, the pseudo-morbid changes, which the different textures of the body undergo after death, have not received the universal attention they deserve. We possess but one good English treatise on the subject, namely, a short memoir by Dr Yelloly on one department of it,—vascularity of the alimentary canal, (Med. Chir. Transact. iv.); and although his remarks have lessened in some degree the laxity of British pathologists in pronouncing upon the existence of marks of disease in the dead body,—much still remains to be done to diffuse correct ideas respecting their imitative or pseudo-morbid signs.

The authors do not lay claim to originality in pointing out the appearances which form the topic of their memoirs: much of what they have stated they acknowledge is to be found

in detached notices scattered throughout the works of modern pathologists. But they claim with justice the merit of being the first to lay down determinately the circumstances under which the appearances in question occur, and the causes by which they are produced. The results at which they have arrived are drawn partly from an extensive set of experiments on the dog and the horse, partly from careful dissections of the human subject after diseases in which the organ examined was presumptively in a state of health. We must add that, in their observations on the human subject, they have been anticipated upon many points by an elaborate work by M. Billard on the healthy and diseased appearances of the alimentary canal, a work to which we shall refer occasionally in the present analysis.

The first section of their inquiry treats of the *Redness of the blood-vessels and lining membrane of the heart*. Coloration of the inner membrane of the blood-vessels and heart has been laid down by many recent writers, not only as designating inflammation, but as explaining symptoms antecedent to death. The authors have quoted the works of Frank, of Bertin and Bouillaud, of Andral, and of Tanchou for examples of the kind; others might easily be pointed out in the writings of their countrymen; and though similar statements have not been made in any modern English work, we have repeatedly heard persons of experience and authority in this country make them in actual pathological dissections. The simple fact of the great variety of diseases in which this supposed inflammatory redness has been remarked throws doubt on the accuracy of those who have noticed it; for it has been found in cases of fever, small-pox, peripneumony, poisoning with arsenic, tetanus, hydrophobia, &c. Accordingly, the following analysis of the researches of MM. Rigot and Trousseau will prove that the redness in question is a mere pseudo-morbid phenomenon.

They set out with an account of the healthy appearance of the inner membrane of the heart and great vessels of the horse, as determined by twenty-eight dissections immediately after death. In every case they found the lining membrane of the heart thin, white, transparent, and here and there of a pearly lustre;—that of the arteries yellowish-white, and that of the veins more purely white. In two instances only the aorta exhibited a few copper-coloured specks. The appearances were not materially different if the examination was delayed till one, two, or four hours after death; and sometimes little change had taken place in five or ten hours, even when the circumstances were favourable to the production of pseudo-morbid redness. But much more generally, both in the horse and in the dog, if the examination was delayed till five, ten, or fifteen

hours after death, the inner membrane of the heart and its great arteries and veins was found speckled or streaked with red stains, which, in the arterial system, were scarlet in tint, and in the veins reddish-brown.

The cause of these colorations was evidently *dying of the vessels in the fluid part of the blood*. For, in the first place, the very same colour was produced by steeping portions of the vessels in it; secondly, the colorations were found only in the vessels of the more depending part of the body; never, for example, in the vessels of the extremities, if the body was kept lying on the back, with the extremities elevated,—but very regularly in the vessels of the depending limbs, if the body was placed on one side; thirdly, the redness was remarked only where there was blood in contact with the vessels, and the speckled appearance evidently arose from some parts of the coats being protected by the contact of the white, fibrinous part of the blood; and, lastly, the greatest coloration was found when the mode of death was such as is followed by the greatest fluidity of the blood, for example, when the animals were hanged.

In accordance with these unanswerable facts, it will be found, that in the human body the greatest coloration takes place when the blood is most fluid, and that at every red speck or streak a portion of blood is to be found, if accidental causes have not made it shift its place. MM. Rigot and Trousseau have related several pointed cases of the kind, and the experience of every careful pathologist must supply him with daily proofs to the same effect. We may add a fact, which we have thrice had an opportunity of witnessing, that, as in animals, so in man the healthy colour of the inner membrane of the great arteries, for at least an hour and a-half after death, is a uniform, faint yellowish-white.

In making observations on the coloration of the great vessels, it is necessary to keep always in view the accidental causes which may remove the blood from one place to another in the dead body. Thus, after having stained the coats of the vessels in the abdomen, it may be squeezed out of them by the subsequent dilatation of the intestines with gases, or during the examination to which the parts are subjected by the anatomist; or, after having been propelled by the former of these causes into the chest, it may suddenly recede when the pressure on the abdominal vessels is removed by the cavity being laid open.

The coloration now described is precisely the same as that which has been referred to by the pathologists mentioned above, as furnishing evidence of an inflammatory state of the vessels. None of them have ever been able to point out any other con-

comitant effect of inflammation, such as thickening of the coats or effusion of lymph, or even to refer the redness to vascularity. It appears exceedingly doubtful, whether simple redness can ever be assumed as proof of the existence of arterial or venous inflammation.

The second section of the inquiry of MM. Rigot and Trousseau relates to the *Redness of the gastro-pulmonary mucous membrane*.

Few dead bodies are opened without more or less local injection of vessels being found in the alimentary canal. This injection is sometimes so great as to form not only a delicate network of vessels, but even actual extravasation of blood into the textures of the parts, or effusion of its red particles upon their surface; and such appearances are frequently found when a determination of blood to the part was not indicated by any symptom during life. It is well known to most pathologists, at least in this country, and particularly since the remarks of Dr Yelloly on the subject, that the redness now alluded to, when unattended by other derangements, cannot be always referred to inflammation, and in fact is generally a pseudo-morbid phenomenon. But since the doctrines of M. Broussais became so unworthily fashionable in Paris, and even gained credit with many of the younger members of the profession in Britain, Dr Yelloly's instructions seem to have been lost sight of. It may therefore serve materially to establish the principles laid down by that author, if we take a short survey of the more extended and minute researches of his successors, MM. Rigot and Trousseau,—who, by the bye, have not deigned to acknowledge his prior and accurate investigations.

Comparative observations on animals are not quite so well fitted for settling the nature and extent of pseudo-morbid infiltrations of the alimentary canal, as for settling the nature and extent of vascular coloration. But nevertheless they throw much light on their cause, and prove satisfactorily that they may be produced to as great an extent as to resemble exactly *all forms of inflammatory redness*. In the dog, immediately after death, the villous coat of the intestines has everywhere a red colour; which, where the membrane is in contact with its own mucus only, has a brownish or violet tint, and a livelier rose or even scarlet tint, where it is in contact either with alimentary matter, or with the air. The stomach has a much paler rose red hue: we have likewise seen it occasionally dull white. In the horse, the natural colour of the mucous membrane of the stomach and intestines is a dull white, with a shade of yellow, and is hardly ever red. The authors have not had an opportunity of examining the natural state of the human

alimentary canal. We are enabled to supply this deficiency in their inquiries, having had occasion to examine the whole intestinal canal in the human subject within an hour and a half after death. It was every where of a dull white colour, although the person did not die of hemorrhage or extenuation.

In the dog and horse no material change is observed, if the examination is made while the peristaltic movements continue. But after that the blood begins to accumulate in particular parts. Even so early as an hour, and always two or three hours after death, some parts of the intestines will be found bloodless, while the mesaraic vessels of other parts are turgid. Different degrees of turgescence are met with. Sometimes the injection of vessels is confined to the submucous cellular tissue, and does not affect the mucous coat itself, although in the former situation it is so minute as to rival the finest artificial injection, and is even accompanied with lenticular extravasation into the tissue. Sometimes, however, in the horse, and still more frequently in the dog, the villous coat itself is of a uniform bright red colour. And occasionally the colouring matter of the blood is effused on the surface so as to give a red tint to the mucus with which it is besmeared.

The several varieties of pseudo-morbid injection have been very minutely and correctly laid down in the work of M. Billard. He divides them into the *Ramiform* injections, which are sufficiently described by the name;—the *Capilliform*, which consist of an intricate net-work of fine vessels, sometimes so close as at first sight to appear an extravasated patch;—the *Punctated*, which resemble the appearance of a moistened membrane, dusted over with a red powder, and which the author erroneously, as we conceive, imagines to be a pseudo-morbid appearance *only* when produced by scraping the villous coat with the scalpel; the *Striated*, which he does not define exactly, but which are named from the arrangement of the redness in streaks, and which are formed sometimes of a finely injected net-work of vessels, sometimes of actual extravasation: the *Laminated*, which are either lenticular, like petechiæ, or in larger patches,—are produced by extravasation under the villous coat,* or incorporation of blood with its substance,—and are often accompanied with coloration of the mucus lying upon them: the *Diffuse*, which are merely an aggravated form of the laminated variety.

* He has related a striking case of this description, in which there was “a large amaranth-red patch, six finger-breadths in extent, situate on a part of the ileum which lay in the right iliac fossa. The mucous membrane was thick, soft, easily torn, and when squeezed gave out blood of a gelatinous consistence.” A similar and larger patch occupied a part of intestine in the pelvis. The person had hung himself. The author seems to view the lenticular or petechial extravasation as always morbid; but we have certainly seen it when disease was out of the question.

These injections and extravasations, continue MM. Rigot and Trousseau, are always most distinct in the most depending turns of the intestines, and in the most depending part of each turn; they are therefore most distinct near the loins and in the pelvis, if the body has been kept on the back; but are, on the contrary, most distinct in the anterior part of the cavity, if the body has lain on the belly. The parts of the intestines which are superior in position are always much paler, often quite pale and bloodless. A portion thus rendered pale may often be as minutely injected as with a fine artificial injection, if its position is reversed in regard to a part of which the mesaraic vessels are turgid with blood. In an injected portion of intestine, those parts are most intimately injected, which are distended with alimentary matter: those which are empty and contracted may be almost pale, even though depending in position. When the injection is very great, the villous coat is evidently somewhat thicker than where it is pale.

Besides the appearances now described, another variety of pseudo-morbid redness is seen at times in the intestines,—namely a red stripe, wherever the intestines are in contact with one another. This variety may be present when the other is trifling. The coloration is seated in the muscular coat, and is disposed in lines perpendicular to the axis of the tube. The authors were unable to determine whether it arises from injection of vessels, or effusion between the muscular fibrils.

It is unnecessary to detain the reader by a comparison of the above appearances with the local redness which is often found in the intestines of man, and which is often hastily imputed to inflammation. Its characters are the same, its seat and extent are the same, and the circumstances that regulate its production, so far as they can be ascertained, are the same. It is impossible, therefore, to avoid tracing it to the same cause,—percolation of the blood towards the most depending parts. The red stripes caused where the intestines lie in contact with one another bear in particular a striking resemblance to the effects of inflammation.

The pulmonary mucous membrane, like that of the alimentary canal, is of a dull white colour, when examined in a healthy body immediately after death. It is liable to two varieties of pseudo-morbid coloration. The one is caused by injection of vessels with blood percolating towards the most depending parts of the tissue. This variety is often seen forming a longitudinal band on the posterior part of the trachea and greater bronchial tubes. The other is analogous to the coloration of the inner coat of the arteries caused by their being dyed in the fluid part of the blood, and is observed chiefly in those portions of the lungs which are gorged in consequence of their depending position. Both varieties are frequently accompanied with

effusion of red particles of the blood, and consequent redness of the bronchial mucus. The tinging of the membrane and its mucus, combined with the turgidity of the pulmonary tissue around, is often mistaken for the effects of inflammation, to which indeed it bears a very close resemblance.

We may add a few remarks from the observations of the authors on this pseudo-morbid turgidity of the lungs. It is observed more or less in almost every subject. Its cause is evidently percolation; for, on the one hand, if, when it is considerable, the position of the lung be reversed, the pale portions will become somewhat turgid in their turn; and on the other, the seat of it is regulated by the position in which the body has lain after death. It is always accompanied with diminished crepitation, sometimes with the total absence of it. It is likewise always accompanied with diminished cohesion, so that the tissue of the lungs is easily torn.—One variety of it occurs even during life. Laennec ascertained by the stethoscope, that in diseases which end in a long stage of agony, the posterior part of the lungs becomes impervious to the air some time before death; and our authors have traced a connexion between this pathological state and the appearance of lividity on the posterior surface of the body under the same circumstances. In all such cases the gorging of the lungs, and likewise of the depending parts of the intestines, is very great. The cause evidently is the insufficiency of the feeble action of the heart to counteract the gravitation of the blood.

MM. Rigot and Trousseau have promised to lay down, in a future memoir, the rules for distinguishing the pseudo-morbid appearances described above from the morbid appearances with which they may be confounded.

ART. V.—*Observations on the Surgical Pathology of the Larynx and Trachea, chiefly with a view to illustrate the affections of those organs which may require the operation of Bronchotomy; including remarks on Croup, Cynanche Laryngea, Foreign Bodies in the Windpipe, Wounds, &c.* By WILLIAM HENRY PORTER, A. M. Member of the Royal College of Surgeons in Ireland, Surgeon to the Meath Hospital, and County Dublin Infirmary, and to the Dublin General Dispensary; and Lecturer on Anatomy and Surgery in the Medico-Chirurgical School, Park Street, Dublin. London and Edinburgh, 1826.

THE liability of the air-passages to disease, to injury, or to

accident, the extreme urgency of the symptoms with which their diseases and injuries are attended, their rapid progress and frequent fatality, are circumstances which invest them with the highest importance, and irresistibly demand the attention of the practitioner of the healing art. The study of these affections, though equally incumbent on the physician and on the surgeon, is rendered most indispensable to the latter, in consequence of the frequency with which his interposition becomes necessary, as a means of saving the life of the patient. It would be idle in the extreme to attempt to estimate the comparative importance of physic and surgery in detecting and treating these complaints. It may be justly remarked, that in no class of diseases is the distinction between the pure physician and the surgeon, strictly so called, more completely lost sight of; and in none is it of so much moment for the physician to derive beneficial guidance from surgery, or for the measures of the surgeon to be regulated by sound medical knowledge.

In the present work, it is the principal object of Mr Porter to investigate the pathology of the larynx and trachea, as their morbid states become the object of surgical treatment. In this point of view it possesses great merit, and is entitled to very considerable commendation. The author has evidently studied the pathological changes incident to the windpipe and its appendages with great assiduity and minute attention; and though his researches and observations have been more especially directed to the determination of the circumstances demanding surgical management, he has, nevertheless, added considerably to the mass of useful medical knowledge, and has produced a work which cannot fail to be equally useful to the medical practitioner and the surgical operator.

Mr Porter begins with some judicious general observations on the pathological changes incident to the laryngeal, tracheal, and bronchial mucous membrane, in the course of which he adverts to the influence of the submucous tissue in causing disease, and considers the circumstances under which the exudation of lymph, of blood, of morbid mucus, or of puriform fluid ulceration, and the formation of sloughs, may respectively take place. He then proceeds to the more immediate discussion of his subject, under the two general heads of *idiopathic affections* of the larynx and trachea, and *affections* of these canals *originating from mechanical injury, or the operation of chemical agents*. Under the former head he enters minutely into the pathology of croup in all its forms and varieties; of inflammation of the larynx, acute and chronic; of laryngeal consumption; of sloughing or death of the laryngeal cartilages; and the effects of abscesses and tumours compressing the canal and im-

peding the transit of the air. Under the head of accidental injuries, he examines the effects resulting from swallowing boiling water, or any of the concentrated acids; those occasioned by the presence of foreign bodies within the windpipe; and the effects of wounds inflicted by accident, or intention, chiefly in attempts to commit suicide. It would be superfluous to follow the steps of Mr Porter in his attentive discussion of each of these subjects; for his observations, though judicious, and entitled to the serious attention of the pathological reader, cannot be reasonably expected to be always new. One or two topics, therefore, only shall be selected as the subject of remark.

Croup.—Acute croup is regarded by Mr Porter as an inflammatory disease occurring in a particular structure, and tending, by three stages, to a given termination. In the commencement the disease consists in incipient inflammation; and the intention is to subdue morbid action, and to prevent the formation of an albuminous membrane within the larynx or trachea. The second stage he distinguishes by the secretion of lymph; and the object then is to procure for the air an artificial passage, which may at once preserve the lungs from congestion, and allow repose to the larynx, while the adventitious membrane is detached and expelled. The third stage he considers to be that in which, from impeded respiration, the brain becomes loaded with unrespired blood, and its functions are impaired and gradually extinguished.

In applying these distinctions to the admissibility of the operation of bronchotomy as a means of saving life, Mr Porter takes the same view which Dr Cheyne does. Though the effusion of lymph, he justly remarks, is generally confined to the larynx, in a number of cases, nevertheless, inflammation commences in the bronchial cells and proceeds upwards. As there is no mode therefore of distinguishing accurately the original seat of the disease, and as an opening made in the larynx or trachea cannot facilitate the admission of air to the lungs, in cases in which the bronchial cells are loaded with viscid exuded matter, the operation under such circumstances is entirely out of the question. In the case of spasmodic croup, the occurrence of which Mr Porter sees reason to admit, (p. 29,) he thinks it offers means of resuscitation; but the suddenness with which infants in some instances recover, and with which they may be cut off, makes it appear doubtful to him how far surgical interference may be serviceable. In short, Mr Porter is averse to the operation in children, and thinks that in cases where death seems to take place from sudden interruption of respiration, the safest plan is to attempt to inflate the lungs by impelling air, by means of an elastic gum tube, through the nostril into the windpipe.

Laryngitis oedematosa.—By this term Mr Porter understands that variety of laryngeal inflammation in which the mucous membrane of the larynx, more especially of the *rima glottidis*, assumes a swelled, glistening, but pale-blue appearance, in consequence of infiltration of the submucous cellular tissue. The name is not very judicious, as it may lead to the notion that the affection thus designated is peculiar and different from all others incident to the larynx. In general it may be viewed as merely one of the effects of inflammation of the larynx; and the perfection in which it appears, will depend first on the proximity of the morbid action to the *rima glottidis*, and secondly on the stage which the inflammation has attained. If indeed Mr Porter's account be intelligible, he applies the term of *laryngitis oedematosa* to ordinary laryngeal inflammation, of which, however, it is merely one of the morbid appearances.

It is chiefly in this disease, and in the case of the effect now mentioned as resulting from it, that the operation of bronchotomy becomes at once a remedial and a necessary expedient. Mr Lawrence may be regarded as the first surgeon who investigated the circumstances indicating this operation; and the excellent essay of this author in the Sixth Volume of the London Medico-Chirurgical Transactions, may still be referred to as containing the most accurate and useful information on the circumstances which render bronchotomy necessary. Mr Porter, after pointing out the urgent necessity of the prompt and energetic employment of the antiphlogistic regimen, recommends the early adoption of the operation, wherever the inefficacy of the usual remedies and the suddenness of the fits of difficult breathing indicate, that the impediment depends on obstruction of the air-passages. He insists, however, on its early performance, which alone, he contends, can render success probable. If it be delayed till the difficult respiration by operating on the bronchial cells and the pulmonary circulation induces the congestion of asphyxia, and consequent extinction of the functions of the brain by the application of unrespired blood, though the operation may not render the patient's condition worse, yet its failure under such circumstances brings discredit on surgery.

Chronic laryngitis. Under this name Mr Porter includes "all those affections of the larynx which materially impede respiration, but which commence so insidiously, and proceed so slowly, as often to produce incurable disease before the patient's attention is awakened to the danger of his condition." This is perhaps rather vague and deficient in precision; but, in the subsequent observations, the views given become somewhat more definite. So far as the morbid changes are concerned, Mr Porter gives his reader to understand, that the mucous membrane

may be thickened to such an extent as never to recover its healthy structure ; that it may be ulcerated ; that the cartilages may be degenerated, that is to say, partially converted into bony or earthy matter, and undergoing exfoliation ; or that they may undergo the process of inflammation terminating in mortification. Again, the submucous cellular tissue, or that among the laryngeal muscles, or even the cellular tissue exterior to the larynx, may become inflamed, and, terminating in suppuration, proves another though a more remote cause of chronic inflammation of the larynx itself.

The affection, however, to which the name of *chronic laryngitis* is most properly applied, is represented as approaching in the following manner. After exposure, while warm, to cold, humid air ; or after great or unusual exertion of the voice, as in long-continued speaking, crying, or singing, the voice becomes hoarse and husky,—in so far as the larynx is concerned, is entirely lost, and may continue so for some days, probably in consequence of an inflamed and slightly thickened state of the laryngeal mucous membrane. Under the use of remedies or spontaneously this generally goes off ; but from carelessness or mismanagement on the part of the patient, is reproduced in consequence of fresh exposure to one or more of the original causes. The voice becomes again hoarse, with a dry husky cough, and a sensation of tearing pain about the thyroid cartilage, which is aggravated by attempts to speak above a whisper, or to swallow. In some instances these sensations may be attended with shivering, headach, quick pulse, and great uneasiness in the windpipe during the night. All these symptoms depend, according to Mr Porter, on a permanent chronic action of the laryngeal membrane, which becomes thickened and more or less red. In this state of matters he has been in the habit of using mercury, he states, with the happiest effects ; though he admits, that, where the disease has been of long standing, while the other symptoms disappear, “ the power of articulating clearly,” he should have said, “ the faculty of laryngeal voice” is never again recovered. This suggests the remark, that cases of this kind of disease are found to be more benefited, first by a full bleeding from the arm followed by the repeated application of leeches in the neighbourhood of the *pomum Adami*, with the internal use of antimonial medicines ; and secondly, by the frequent use of blisters over the throat, and the insertion of a seton under the skin.

Notwithstanding the use of all these means, however, this disease may go on, and by its obstinacy become highly formidable, and bring the patient into the most imminent danger. From some cause observed or unobserved, the breathing becomes suddenly

very difficult and laborious; and, during the ineffectual efforts to inspire, the trachea and larynx are drawn as high as possible on the neck, and the digastric muscles are seen in strong and violent action. The symptoms, in short, are quite similar to those of *laryngitis oedematosa*, (acute laryngeal inflammation.)

In this form and stage of the disease, Mr Porter thinks bronchotomy particularly called for, and if seasonably employed, very rarely employed in vain. "The disease is caused by a mere thickening of the mucous membrane, without any morbid alteration of structure; but the circumstance of the larynx being in constant use tends to maintain the action that is going forward, and, finally, if not relieved, to produce such thickening of the part as will be incompatible with the maintenance of its functions. It is thus that, creating an artificial passage for the air, operates in promoting recovery; and there is reason to believe that after the operation, the power of nature would be sufficient to work out a cure, even without the intervention of medicine, merely because the organ can enjoy repose." p. 124.

Laryngeal Consumption, (Phthisis Laryngea.) To this head must be referred all the other forms of laryngeal disease enumerated in the last section, viz. ulceration of the membrane, abscess in the submucous or intermuscular cellular tissue, abscess complicated with disorganization of one or more of the laryngeal cartilages, and abscess with mortification of the laryngeal cartilages.

In *abscess in the submucous or intermuscular cellular tissue*, which is in some cases of the nature termed *diffuse inflammation* by Dr Duncan Junior, the practice observed by Mr Porter is that of making a deep incision down to the trachea; and though the matter may not at first be thus reached, it never fails to appear in the wound in the course of a day or two, on the same principle as that which regulated the practice of Dr Graves, as mentioned in our last number, (p. 318.) The observations on the usual site of this sort of abscess, and on the mode of ascertaining its presence, are extremely interesting, and deserve the attentive perusal of the surgical reader.

Abscess, with earthy disorganization of the laryngeal cartilages.—This severe and generally incurable disease, Mr Porter regards as the result of a morbid change which the cartilages undergo, according to his observation, about the age of thirty-two, and between that and thirty-six. This process is preceded and accompanied with marks of high organization in the structure of these substances, the section of which, when divided, is then red and vascular. It commences most commonly in the posterior broad part of the cricoid cartilage, near the centre of which a small hard gritty patch, whiter than the neighbouring red part, is depo-

sited. This is progressively enlarged till it occupies the greater part or the whole of the cartilage, when, from its inorganic nature, and the high sensibility of the surrounding and neighbouring parts, it operates as an extraneous irritant, and gives rise to inflammation, which soon terminates in abscess. When the matter is discharged, which it does by several openings, gritty or earthy exfoliations take place; and the copious but fetid expectoration, and bad breath, soon announce the hopeless nature of the malady. The original source of irritation continues to operate; the patient is worn down with cough, difficult breathing, and incessant irritation of the air-passages; and he expires with most of the symptoms of hectic fever.

In this state of misery the business of the medical attendant is chiefly palliative; and the surgeon, though he is assured of the nature of the disease, need scarcely make an attempt to save his patient. Mr Porter, though he states that he has prolonged a life of misery for a month by performing the operation of bronchotomy, and is aware that others have been still more successful, admits that a small addition to existence procured on such terms is no advantage, and dissuades the surgeon from performing the operation, unless under very peculiar circumstances, and at the urgent solicitation of the patient or his friends.

Abscess, with mortification of the laryngeal cartilages.—Of this Mr Porter has met with two instances, one of which was acute, and run its course with great rapidity; the other chronic, and proceeded so slowly as to disguise its real nature, till the operation of bronchotomy was performed. The cartilages in this complaint are red, but without distinct vessels in their substance, less dense than in the healthy state, and become spongy by maceration. When the action is completed, they appear black and dissolved like wetted or rotten leather, (p. 117.) This disease gives rise to secondary suppuration of the laryngeal cellular tissue; openings take place at one or more points, in some instances into the œsophagus; and when the air is admitted, the cartilage is seen lying at the bottom of the sore, of a brown colour, shrunk and shrivelled like horn which has been exposed to the fire. The dead cartilage, in such circumstances, emits a most fetid smell, which, though different from that of carious bone, is equally offensive, and is almost pathognomonic of the nature of the disease.

Mr Porter has not met with any description of this disease by authors; and the two cases which have fallen under his personal observation do not enable him to speak of it with much certainty. As cartilage is not authentically known to admit of reproduction, he infers that the larynx must be permanent-

ly destroyed ; and that existence can be maintained in no other mode than by establishing an artificial opening during the remainder of life. The wasting and fatal influence of the constitutional symptoms, and of the local irritation, however, hold out no great encouragement to this measure. This conclusion, indeed, is in general applicable to most of those forms of laryngeal disease, which have been shown to give rise to phthisical symptoms.

On the subject of foreign bodies in the larynx and trachea, Mr Porter has entered into a very full and comprehensive inquiry, and illustrated his views by various apposite cases, either from personal observation or from respectable published authorities. It is needless here to enter into any lengthened account of this part of the work of Mr Porter. It is sufficient to say, that he takes great pains to point out all the circumstances which may modify the effects of this accident, whether from the shape of the foreign body, or the situation in which it is fixed ; and to distinguish the cases in which the operation is to be beneficial from those in which it is likely to be unavailing.

The last subject of consideration in the volume is that of *wounds of the larynx and trachea*, and more especially the operation of bronchotomy itself, which the author contrasts with wounds inflicted with the intention of suicide. Bronchotomy, he thinks, has been hitherto rather an unsuccessful operation, because it has been performed unseasonably late, whether for saving life during an attack of laryngitis, or for the extraction of a foreign body from the trachea. In other instances its want of success has been simply negative, that is, when it has been performed in cases of organic change of structure quite incurable, and when consequently it could not be expected to be more beneficial than any other remedial measure. These objections are certainly not essential to the operation, and are of that kind which more varied experience, greater deliberation, and more patient attention, might unquestionably obviate entirely.

Another difficulty, however, perhaps more important than these, has been very properly adverted to by Mr Porter in his examination of the effects of any incision into the windpipe. This consists in the effusion of blood from the wound, sometimes in such abundance as to flow into the windpipe, and if not occasion instant death, at least be the cause of very threatening cough and suffocation. In a large proportion of cases this accident becomes infinitely the most serious inconvenience of the incisions of bronchotomy ; and if it be not so frequent as to merit the opprobrium of an essential objection, it takes place at least under circumstances where very great caution and consummate dexterity are not always successful in preventing its occurrence. It is doubtless

true that the performance of the operation in the crico-thyroid membrane, as proposed by Mr Lawrence, diminishes, but does not remove the chance of this inconvenience. Nor is it always very practicable to tie the vessels, at least after the opening into the windpipe has been completed. It is therefore of some moment to state, that the perpendicular incision is much less liable to this inconvenience than the transverse one; since the vessels are not only much less likely to be divided, but may, by cautious dissection, be pushed aside, while the operator proceeds, by successive steps, to complete his opening into the larynx. It is not hemorrhage merely which he has to fear in such circumstances, but the effusion of blood into the windpipe, so as to cause a greater evil than that which it is his purpose to remove, and render instantaneous and certain the fatal event, which, though near, was neither immediate nor inevitable. On this point the remarks of Mr Porter at pp. 238, 239, and 249, will be perused with advantage.

The foregoing hasty sketch of the principal subjects discussed in the present volume, though not intended as a complete account of the work of Mr Porter, will perhaps communicate some idea of the nature of its contents, and of the manner in which the author has treated them. It is possible that in the account now given the language of panegyric has been less liberally used than the merits of the work may seem to justify. In answer to this it may be very safely said, that its intrinsic value is really so great, that to be justly appreciated it requires only to be attentively perused, and the best encomiums will be derived from the spontaneous suffrage of the members of the profession. The general conclusions are illustrated by the detail of several appropriate and instructive cases; and what is deficient in the former is amply supplied by the latter. Upon the whole, little doubt can be entertained that the treatise of Mr Porter will speedily secure for itself the rank of a standard work, and for its author the character of an able writer on surgical pathology.

ART. VI.—*De la Membrane Muqueuse, &c. On the Gastro-Intestinal Mucous Membrane in its Natural State, and in a state of Inflammation; or Pathological Researches on the various Healthy and Diseased appearances of the Stomach and Intestines.* By C. BILLARD, Hospital Assistant of the Infirmary of Angers, &c. Paris, 1825. 8vo, pp. 565.

THIS work, which we should have noticed sooner, obtained for its author the prize proposed by the Parisian Athenæum of

Medicine in 1823 for the best practical treatise on the various appearances which the alimentary mucous membrane presents in a state of health and in a state of disease, and on the mode of distinguishing its morbid and pseudo-morbid appearances from one another. It is founded almost entirely on personal observations made for many years in the wards and anatomical theatre of a great hospital; and certainly appears to possess the peculiar merit, claimed for it by its author, of having been written without reference to any theory. Such a work cannot fail to interest every pathological physician, and to correct and improve many vague notions at present entertained regarding the pathology of the stomach and intestines.

M. Billard sets out with an account of the colour, rugæ, villosity, muciparous glands, and natural secretions of the stomach and of the small and great intestines, in a state of health, and at different ages.

The *Colour* of the mucous membrane in the adult state, when not injected either by disease or by pseudo-morbid infiltration, is in the stomach dull-white, in the duodenum and jejunum pale ash-gray, in the ileum grayish-white, and in the great intestines white. In the foetus, however, the whole alimentary canal has a pale rose-red tint, which disappears very soon after birth. During digestion too it assumes in all periods of life a more or less vivid red blush at the points where the alimentary matter is undergoing the process of chylification.

The *Rugæ* differ very materially in distinctness, even in circumstances apparently the same. Stomachs, for example, are occasionally found in which, although they have not been distended, scarcely any rugæ are visible. The first inch, or inch and a half, of the duodenum is free from rugæ or valves; the rest of the duodenum is crowded with imbricated plates, which become fewer and less prominent as we proceed downwards, till at length, at the termination of the ileum, they are merely linear. The valvulæ of the intestines do not begin to be formed till the seventh month after impregnation, the rugæ of the stomach not till birth.

The *Villi* of the stomach and intestines are sometimes hardly perceptible, occasionally very distinct, even without the aid of a microscope. They are best seen by floating the membrane under water, and then passing the finger gently backwards and forwards over its surface. They are most abundant in the stomach and duodenum, diminish in number and distinctness in the rest of the small intestines, and in the great intestines are hardly visible.

The *Muciparous glands*, about which anatomists are not yet very generally agreed, have been studied very carefully by M. Billard, and the following is the result of his inquiries. Some-

times they cannot be discovered but with great difficulty ; in other bodies, although the membrane is quite healthy, they are so distinct that even their orifices are visible to the naked eye. They are most obvious in the duodenum, at the termination of the ileum, and in the great intestines. In their most indistinct form they are seen arranged in oblong laminae, with a barely perceptible line bounding them, and causing an increased opacity of the mucous coat when it is held between the eye and the light. In other places they are rendered more distinct by constituting little white granulations as big as a millet seed, and communicating together by a fold of the membrane. In their most distinct form their surface is studied with such granulations, the summit of each of which has a little gray point. In this form they are seen in the lower part of the ileum.

It is impossible to give an accurate idea of the *Thickness* of the mucous membrane of the intestinal canal in a state of health. It is thickest in the duodenum, then in the stomach, next in the rectum, jejunum, and ileum, and thinnest in the colon. When a fragment torn from the subjacent coat is stretched over the finger, the finger is seen through it as through white crape.

The *Mucus* secreted on the surface of the membrane is abundant in the foetus, and very firm about the time of birth. It is not easy to lay down its healthy characters in adult age ; but it is never red, as some suppose, unless where the membrane beneath is infiltrated with blood. It becomes less abundant in old age, particularly in the great intestines.

Having thus described the principal appearances of the stomach and intestines in their natural state, M. Billard proceeds to determine the various appearances which their mucous membrane assumes when in a state of inflammation ; and he considers these appearances under two heads, changes of colour and changes of structure.

Section I. *Of Inflammation as denoted by change of colour.* The changes of colour he has remarked are of four sorts, red, brown or violaceous, bluish-gray, and black.

1. *Redness*, produced by minute injection of the capillary vessels of the mucous membrane, may arise from passive injection or infiltration after death, as well as from inflammation ; and it appears that *there is none of the inflammatory varieties of it which is not closely imitated by pseudo-morbid infiltration*. He has particularised six varieties, which he denominates Ramiform, Capilliform, Punctated, Striated, Laminated, and Diffuse redness,—terms which are easily understood, and which, it is to be wished, were introduced generally into pathological language. It is unnecessary to repeat here what has been said

of these varieties of redness in our analysis of the memoirs of MM. Rigot and Trousseau; but we shall now mention the characters by which M. Billard proposes to distinguish the morbid from the pseudo-morbid varieties. These characters are drawn not, as may be readily imagined, from any peculiarities in the redness itself, but from collateral circumstances. According to our author, redness of the intestinal mucous membrane is to be accounted inflammatory only when it occupies a part of the gut not depending in position, or is not limited to that part,—when the veins of the mesentery leading to the part are not distended, and the great veins of the abdomen have not been obstructed at the time of death,—when there is on the reddened membrane an unusual quantity of mucus, either of ordinary consistence and colour, or tenacious, thick, and adhering to the membrane,—when the mucous membrane has become opaque, so that if dissected off and stretched over the finger, the finger is no longer seen as through a piece of white crape,—when the cellular tissue which connects the mucous with the subjacent coat is brittle, so that the former is easily scraped off with the nail. The secretion of an unusual quantity of mucus he considers to be one of the earliest phenomena of inflammation in this as in other mucous membranes. Opacity of the membrane follows next in order, and lastly, brittleness.

2. The *Brown* and *Violet* coloration of the alimentary mucous membrane, M. Billard conceives, can hardly ever be the result of passive infiltration, because it implies not only the passage of blood into the capillaries, but likewise alteration of its properties, and such an alteration too as cannot be produced by physical agents. A vital process is necessary. And indeed he considers that the brown or violaceous coloration indicates not only the pre-existence of an inflammatory action, but likewise its existence for a considerable period before death. It is of two kinds—*uniform* or *checkered*. The following is a characteristic example :

An old woman, after having been long confined at home by ill health, was carried to the hospital of Angers, and died four days afterwards. During life the tongue was covered with cracks and scales, the skin was pungently hot, her countenance was of a straw-yellow colour, and she had epigastric pain on pressure. Her case was therefore considered and treated as one of inflammation of the stomach. On examining the body, a considerable turgescence of the mesenteric veins was found. The mucous membrane of the stomach was lined with a great deal of tough mucus, beneath which the membrane itself was of a deep brown colour, resembling that of tanned leather, and not removable by maceration for twenty-four hours. This coloration extended into the duodenum, and there gradually passed into the natural pale-ash-gray tint of the intestine.

The remainder of the intestinal canal was injected with passive ramiform redness.—(Case xlii.)

Although M. Billard supposes that this variety of coloration always indicates disease, he has related a case which it is difficult to conceive to have been of this nature, as the subject of it was killed by a fall, and was in good health at the time of death.

3. The *Bluish-gray*, or *Slate-gray* coloration of the alimentary mucous membrane is admitted by the author to be occasionally produced, independently of the existence of inflammation, particularly when the passage of the blood through the heart is much obstructed. But still, in nine cases out of ten it has appeared to him to be the consequence of inflammatory action; and he conceives that it arises from changes induced by vital processes on the more ordinary red colorations, and that it therefore, like the former variety, denotes chronic inflammation. The following is an example.

An elderly man, liable for some years to dropsy of the belly, was attacked towards the close of his life with uninterrupted diarrhoea, not accompanied with fever. After death, six pints of serum were found in the peritoneum, the external surface of the intestines was spotted with gray specks, the villous coat of the stomach had a slate-gray colour, could be removed easily in large plates, and was covered with an abundant mucus. The whole mucous membrane of the intestines was of the same bluish-gray colour, and in several places was raised up by the effusion of serum into the cellular tissue beneath. A portion of the coloured intestine was steeped three days in water, after which a punctated gray coloration was still visible, and the water was rather rendered turbid than coloured.—(Case xlviii.)

4. The last variety of coloration found in the mucous membrane of the stomach and intestines is the *Black* or *Melanotic*. This variety is considered by M. Billard to be always the consequence of disease; and, in opposition to the prevailing opinion with regard to the nature of melanosis generally, he seems disposed to consider the melanotic depositions in the alimentary canal as depending on an inflammatory action. The following case is a very interesting example of this singular disease.

A young soldier, after travelling long from hospital to hospital in an infirm state of health, was admitted into that of Angers, where he died in fifteen days, excessively emaciated and affected with all the symptoms of a chronic enteritis. The peritoneum was here and there tuberculated, the intestinal canal much contracted and gray-coloured externally. The villous coat of the stomach was gray, spotted and streaked with black, and here and there striped with violet, but its strength and consistence were not diminished. The mucous membrane of the jejunum was gray, checkered with black points, of the size and form of flea-bites; and it was also soft, so as

to be easily scraped off. The cœcum was violaceous, striped with black, and granulated internally. The mucous membrane of the whole colon, as far as its sigmoid flexure, was thickened, of a gray colour, with large irregular black patches, like those seen on the surface of the lungs; and the partitions of the *lacunæ* were very red, and blood oozed out from them. At the sigmoid flexure, the whole membrane was black. In the rectum there were many red prominences formed by the mucous membrane. The black matter was situated almost entirely in the cellular tissue which connects the mucous with the muscular coat. A little of it scraped together with the scalpel, proved to be inodorous, and did not dissolve, but merely sank in water. A portion of the affected intestine was steeped twenty-four hours in water without having its appearance altered.—(Case liii.)

M. Billard has related another case of the kind, in which death was preceded by chronic enteritis, and has quoted others from the experience of various authors. He is therefore inclined to think, that the melanotic deposition in the intestinal canal is the effect of an inflammatory process of some standing. He concurs with M. Breschet and other recent authors in ascribing the colour to some change wrought upon the colouring particles of the blood. He adds, that the melanotic deposition is occasionally seen in the stomach, in consequence of the introduction of irritant poisons into that organ, and is then clearly produced by the irritation of the mucous membrane, and consequently indicates inflammation. *He appears never to have met with it as a pseudo-morbid appearance.* In this particular our own experience agrees with his.*

M. Billard concludes the present section of his work, by noticing certain appearances which sometimes accompany inflammatory coloration. These are distension, contraction, sanguinolent exudation, hemorrhage by exhalation, hemorrhage by rupture of vessels, and changes in the appearance of the feces.

Distension of the stomach and intestines is frequently remarked when the whole intestinal canal is inflamed; but it may exist independently of inflammation, as in the cases of ileus supposed by Dr Abercrombie to arise from loss of muscular power in a part of the intestinal canal; and still more frequently it is seen as arising after death. Still less importance can be attached to *Contraction* of the canal as a concomitant of inflammation.

* For some observations on this subject, see the original department of this Number, p. 105.

Very frequently the mucous lining of the inner membrane is tinged red, by the *Exudation* after death of the colouring particles of the blood, where it is accumulated in the coats by passive infiltration. This appearance can never be assumed, as is done by many, for a sign of the pre-existence of inflammation, unless other more unequivocal signs are present. It must be distinguished from proper *Hemorrhage*, which may take place, on the one hand, by rupture of vessels caused by ulceration of the intestines themselves, or by the discharge into them of blood from adjoining organs,—and on the other, by exhalation from the mucous membrane. Exhalation of blood most frequently occurs as a passive affection, being caused by the recession of blood from the external surface to the internal organs. But sometimes it arises, as M. Billard conceives, from inflammatory action; of which he has related the following instance:

A stout young man, soon after being cured of a *dartrous* eruption, was attacked with low fever, marked by delirium, great prostration, sopor, and diarrhœa; and he expired on the eleventh day. On the lower third of the *ileum* there was a large red patch, with thickening of the mucous membrane, and numerous ulcers. The cœcum and beginning of the colon were similarly affected: The mucous coat of the transverse colon was thickened, rough, dry, and covered with a stratum of vermillion blood: In the descending colon the blood was mingled with the feces: The whole rectum was filled with clots of blood.—(Case lv.) M. Billard thinks the blood proceeded from the inflamed transverse colon by exhalation.

The *feces* in inflammation are sometimes thick and hard, sometimes more fluid than usual. The former state he considers to be either its cause, or at all events its antecedent; the latter is the effect of inflammation of the mucous membrane.

Section II.—Under the second section of the work are comprehended *Appearances chiefly indicative of inflammation, and which consist in changes of structure.* These are divided into two orders, changes without loss of substance, and changes with loss of substance.

1. The order comprising *Changes of structure without loss of substance*, includes emphysema, œdema, fungus, hypertrophia, polypus, extenuation, softening, enlargement of the muciparous glands.

Emphysema of the mucous membrane is believed by M. Billard never to be the result of an inflammatory process; but, from whatever cause it may proceed, it is sometimes seen when putrefaction will not account for it. The appearance it assumes is that “of little bubbles of air, irregularly clustered, whitish in colour, soft but elastic in consistence, crepitating when touched,

and altogether resembling the froth of soapy water." We have observed this appearance in connexion with air-bubbles in the blood of the heart, and in the vessels of the brain,—and have been accustomed to consider them all as phenomena taking place after death.

Œdema may be inflammatory or passive. Sometimes, in cases of organic disease of the heart, the mucous membrane of the stomach or intestines is seen elevated, even so as to form little blisters, easily separable, undermined by serous effusion in the submucous cellular tissue,—but at the same time transparent, white, with blue veins traversing it, and without redness. But a more limited degree of *œdema* is also occasionally found as the consequence of inflammation. In a case of phlegmonous erysipelas ending fatally with enteritic symptoms, Billard found in the intestines various signs of inflammation, and among the rest, "when the submucous cellular tissue of the *jejunum* was pricked, and the gut pressed, little drops of serum were squeezed out, and the surface of the mucous coat was raised here and there by the fluid into clusters of little eminences, like the papillæ of the tongue, and was opaque."

Fungus of the alimentary canal is a vague term used by our author to denote the appearance of granulation in the alimentary mucous membrane. This appearance may be passive,—and then arises from some obstruction to the return of the blood through the great veins of the heart. Thus, in cases of diseased heart M. Billard has seen the mucous membrane of the stomach of a cherry-red colour, thick, granulated, soft, transparent, and with red vessels ramifying minutely beneath it. The following, however, is an account of the inflammatory variety, which he met with in a phthisical patient. The stomach was contracted. The villous coat of the stomach was covered with a gray pseudo-membrane of great tenacity. The villous coat itself was scarlet-red in colour, irregularly grooved and surmounted everywhere by round, soft granulations, which bled when torn. The inflammatory variety is known by the opacity of the membrane, the want of blood in the mesenteric veins, and the presence of a pseudo-membrane, or of an increased quantity of thick mucus.

By *Hypertrophia* of the mucous membrane M. Billard understands a very rare disease, which he is disposed to consider inflammatory in its origin, and which consists in simple increase in volume without change of structure. In a case of this nature, that of an idiot who died of prolonged diarrhœa, and in whose stomach and intestines the violaceous colorations noticed above were found very distinct, the whole mucous coat of the colon was *four lines in thickness*, red, firm, dry, and lined by a firmly adhering gray-coloured pseudo membrane. It could be

removed in large shreds with facility, and on its free surface the viollosities were here and there remarkably developed.

On the subject of *Polypus* of the alimentary canal the work under review does not bring forward any new facts.

Extenuation of the mucous membrane is also discussed very briefly. Sometimes the membrane is found forming partially or generally a very thin transparent coat, adhering with unusual force to the subjacent coats; but M. Billard has not been able to observe this state in conjunction with the signs of inflammation, or with any other particular morbid process.

Softening of the mucous membrane, without loss of substance, is a very common appearance, and is considered by M. Billard as indicating previous inflammation in every case in which it is not owing to putrefaction. Softening of the inner membrane is sometimes general, sometimes partial, and is known by the tissue separating between the nails, when an attempt is made to pinch it, or by its peeling off when it is scratched. He has noticed one remarkable case in which almost the whole mucous membrane was in this state; it was combined with other decided signs of inflammation, and the subject died of prolonged dysentery. He has likewise made some observations on the softening caused by decay; and from these it appears that, if the mucous membrane is protected from the air, it does not become softened by putrefaction for a much longer period than would be supposed,—that the membrane becomes easily separable before it softens, and that some of the appearances caused by inflammation may be distinguished, although decay has advanced considerably. He appears not to admit that variety of softening which John Hunter ascribes to the gastric juice acting after death.

Of Enlargement of the Muciparous Glands, M. Billard describes three varieties. The first consists of simple enlargement, without inflammation or induration; the second is an acute inflammatory affection; and the third consists in enlargement and tubercular or scirrhus degeneration.

The first is a rare variety, the author having met with it twice only. The glands, when affected by it, differ from their natural state only in being more developed and covered with an abundant mucus. In one of the cases which have been detailed, the disease was marked during life by vomiting, and profuse discharge of white, glairy, frothy matter by stool; but there was no pain or pyrexia. The whole alimentary mucous membrane was interspersed with little grains about the size of millet-seeds, without the slightest appearance of redness; and the lower part of the intestines was filled with a mucous fluid like that discharged during life.

The second variety of enlargement constitutes true inflam-

mation of the muciparous glands. It has been several times described in recent times, and more particularly by MM. Serres and Petit, and likewise by M. Breschet. It has been observed by M. Billard chiefly in cases of *ataxic* fever: others have remarked it particularly after small-pox. In the early stage of the disease the glands are simply swelled, red, and surrounded with a red areola; when farther advanced, their base expands, a white spot appears on their summits, with a gray-coloured depression in the middle of it; and when the disease is fully formed, the tumours become often confluent, and are filled with a bloody pus, or have discharged their contents by ulceration of their surface. In some instances the pustules, when matured, bear a close resemblance to the variolous pustule on the skin. M. Billard has related several excellent cases of this nature.

The last variety is a chronic affection of a tubercular nature, and it occurs chiefly in combination with other tubercular disorders. M. Billard describes it as conjoined with *tabes mesenterica*, tubercular phthisis, and strumous enlargement of the external glands. In the early stage the enlarged muciparous glands are simply indurated, in the advanced stage the outer portion of the little tumours cuts like lard, and the centre resembles plaster of Paris in its pulpy state. All these varieties of enlargement of the muciparous glands are met with most frequently and in greatest abundance in the ileo-cæcal part of the intestine.

2. The next order of appearances include *Changes in structure accompanied with loss of substance*.

The first article under this head treats of *Ulceration of the Muciparous Glands*. In the same manner as we have seen that the muciparous glands are liable to an acute and a chronic enlargement, so these diseases may terminate in acute or chronic ulceration. Acute ulceration occurs at the close of the acute form of enlargement of the glands, and appears to have been found, like that affection, chiefly in *ataxic* or putrid fevers. In its successive stages this variety of ulceration is known first by simple laceration of the summit of the pustule,—then by the extension of the edges, the deposition of a bloody pus on the surface of the ulcer, and the truncation of the apex of the cone formed by the tumour previous to its bursting,—and, lastly, by the resolution of the whole tumour, the destruction of the mucous down to the muscular coat, and by thickened, red, and perpendicular edges. The chronic form of ulceration, as it occurs in phthisis, *tabes mesenterica*, and other disorders commonly called strumous, is known by the edges being generally white, hard, round, turned inwards, and by the surface of the ulcers being often covered, not with proper pus, but with a whitish or

grayish pulp, the remains probably of the tubercular matter. As these ulcers advance, however, they assume more nearly the appearance of the acuter variety. They very frequently exist without any distinct symptom during the patient's life.

Besides the foregoing varieties of ulceration of the alimentary mucous membrane, M. Billard has briefly noticed several forms, which do not commence in the muciparous glands, but in the mucous tissue itself. One of these appears to depend on the deposition of tubercular matter in the form of grains in the submucous cellular tissue, and the subsequent destruction of the mucous membrane by the pressure of the tubercles. Of this variety he has related a distinct example. Another consists of circumscribed softening of the mucous coat, producing a cavity with smooth and sharp edges as if the piece had been taken out with a strap-borer, and subsequently involving the muscular and even the peritoneal coat, so as to cause complete perforation. This form has been particularly described by M. Cloquet.* A third variety commences with superficial excoriation of the mucous coat, without the formation of pus or any of the other customary characters of ulcers of that membrane; but as it deepens, it assumes the appearance of ordinary ulceration, is lined with pus, and surrounded by an elevated and hardened margin. All these forms the author thinks he has traced as affections independent of the muciparous glands.

Under the subject of *Gangrene* of the mucous membrane, the author has selected several excellent cases of the formation of *eschars* and *gangrenous ulcers* formed by natural processes. The following is an example of the former kind:

A woman, whose whole family had died of dysentery, was attacked very violently with all the usual symptoms of that disease; and died of it in twelve days. (Although she was in hospital at the time the disease began, and there was violent fever from the commencement, the treatment consisted in the administration of *twenty-four grains of ipecacuanha in three doses*!) Besides other signs of derangement in the alimentary canal, two large eschars were found on the villous coat of the stomach. They were on a level with the rest of the membrane, and of a grayish-colour, marbled with yellow, were surrounded with a deep yellowish groove, and beyond this line of demarcation the villous coat was red, granulated, and bloody. The muscular coat was unaffected. The gangrenous odour was quite distinct.—(Case lxxviii.)

A similar disorganization was found in the case of a lunatic, in whom there had been no corresponding symptoms during life. The *gangrenous ulcer* of M. Billard is nothing else than the ulcer left after the dropping away of the eschar described above.

* *Nouveau Journ. de Med.* for January 1813.

It is the common appearance presented by the great intestines after death from dysentery, of which he has given some accurate and valuable descriptions.

The foregoing article is succeeded by one or two remarks on *General Destruction* of the mucous coat, on which subject, however, he has brought forward nothing new, or requiring particular remark.

The same observations apply to the concluding article of the work, on *Cicatrix* of the mucous membrane. The author considers that after ulceration the villosity of the mucous membrane is never regenerated, as some pathologists among his countrymen have lately maintained.

The preceding analysis will, it is hoped, convey some idea of the vast quantity of useful, and, in many respects, new information contained in the work of M. Billard, and will probably satisfy the reader that it is well qualified to diffuse correct and precise notions regarding the various pathological derangements to which the mucous membrane of the stomach and intestines is liable. In Britain we believe practitioners are not yet sufficiently aware of the frequency of these derangements, or of the extent to which they occur as the causes of diseases apparently of the general system. The present treatise appears to us well calculated to supply this defect, without leading into the errors and extravagancies of Broussaism. On this account we may express a hope that it will appear ere long in an English dress.

ART. VII.—*Observations upon the Autumnal Fevers of Savannah*. By W. C. DANIELL, M. D. Savannah, 1826, 8vo, pp. 152.

SAVANNAH, the capital city of the State of Georgia, in the southern division of the North American Union, is situate in $32^{\circ} 5'$ north latitude, and $80^{\circ} 20'$ west longitude, and stands on a plain on the south bank of the Savannah River, about twelve miles in a straight line from the sea. This plain, which is composed of sand discoloured in some places by iron, and irregularly traversed by a narrow vein of clay, is raised about forty feet above low water-mark; and the elevated ridge thus formed, after extending upwards of a mile along the bank of the river, terminates abruptly at either extremity, forming what is known by the name of a *bluff* in the nautical language of the North American settlers. To the south of this ridge the ground descends gently and regularly for several miles. Opposite the city lies Hutchinson's Island, the soil of which is alluvial, and being

subject to inundation by ordinary spring tides, is well adapted to the cultivation of rice. The city is bounded on the east and west by similar tracts, employed in the same species of produce. The ordinary elevation of the tides opposite the city is about eight feet. The salt water approaches annually within four or five miles of Savannah; and in 1818, while the river was low, it came to within about half that distance. Soon after passing the city, in its course to the sea, the river is divided into numerous channels by small islands of green marshy land, the result of alluvial deposit from the waters of the river.

The Carolina shore affords a large tract of rice land, most of which is in a state of cultivation. These islands, in consequence of their level situation, are liable to be inundated during storms with from six to ten feet of water, as happened in the years 1804 and 1824; and as the cultivated banks of the river are diversified with mounds of rich black earth rising ten or twelve feet above the contiguous soil, security during these inundations is too frequently obtained by residing in these elevated spots. Experience has shown, according to Dr Daniell, that residence on these heights is more sickly than on the adjoining level grounds; and it appears further, that settlements on the river bank are more healthy than those at some distance.

The city of Savannah itself, being divided by numerous wide streets and lanes, mutually intersecting at right angles, and broken by large squares at regular distances, is open, spacious, and well aired; and its elevated situation above the adjacent country gives it the advantage of a pretty regular sea breeze, which moderates considerably the heat of summers otherwise intense. The sandy nature of the ground on which the city stands, renders it so porous, that in general water never remains in the streets forty-eight hours after its fall. The winters are mild in a remarkable degree; and the atmosphere is said to be so soft and genial, that the climate of Savannah is represented as more favourable to consumptive patients, than that of any other city in the southern states.

Placed in such circumstances, Savannah is represented by Dr Daniell as naturally a healthy situation. But, like other cities of the Union, built either on the sea-coast, or on the banks of rivers, it is subject to epidemic visitations of that disease, which, whether known under the name of *bilious*, of *autumnal*, or of *yellow fever*, is one of the most certain and powerful destroyers of the human race in the New World. As the physical characters of the surface, however, are believed neither by Dr Daniell nor many other persons in Savannah to possess any thing in themselves necessarily productive of this malady, they have laudably undertaken to investigate the true origin of this

occasional sickliness, with the view of effecting its permanent extirpation.

It is to the present character of the lands in the immediate neighbourhood of Savannah, and to the pernicious mode in which the rice-crops are cultivated, that Dr Daniell ascribes the unhealthy seasons with which occasionally its inhabitants are assailed. While these tracts were in a state of nature, disease was unknown; and it is stated on the authority of Dr Jones, that seventy-five years ago, Savannah was resorted to on account of its salubrity by the inhabitants of Charlestown. Since the general introduction of rice-crops, however, in rearing which, it is well known, the low grounds are extensively flooded; tertian and quartan agues are said never to be wanting; bilious fever prevails to various extent and severity every autumn; and in certain seasons the concentrated epidemic spreads far and wide with its worst features, of yellow skin, hemorrhagic effusions, and black vomit. To show how intimate is the connection between this practice of inundating the rice fields, and the prevalence of epidemic disease, it is sufficient to look to the bills of mortality, during the time when it was in full operation. In the year 1815 the mortality is stated at 1 in every 15 persons of the white population; in 1816 it is 1 in 18 persons; and in 1817 it is so high as 1 in $9\frac{2}{3}$ persons; giving an average of about 1 in 11, which is stated to be a fair estimate of the habitual mortality occurring in the white population of Savannah for many years previous, in consequence of autumnal disease only.

It was impossible that a rate of mortality, increasing to such an amount as one in almost every ninth person, was to pass unheeded or unexamined. A number of philanthropic individuals, aware of the facts already stated, and pursuing the same train of reflection with Dr Daniell, had long before conceived the idea of cutting off the sources of fever in the immediate vicinity of the city, by changing the plan of cultivating the low tracts from what is styled the wet to the dry system of culture. In the year 1817, accordingly, it was resolved, at a general meeting of the leading inhabitants, that the sum of 70,000 dollars should be appropriated by the city to purchase the right of wet culture of such of the tide swamps as were adjacent to the city. A contract was formed to put the lands thus acquired under dry culture, at the rate of 40 dollars per acre; and the system was in full operation the following year. The great diminution of the mortality which this year (1818,) was as low as 1 in $62\frac{3}{4}$ is evidently, however, not to be totally ascribed to the influence of this cause; for as seasons of great mortality are almost invariably succeeded by one considerably less, for reasons which are

well known to those familiar with the history of epidemics, so the diminished rate of 1818 seems partly, at least, to be the natural consequence of the great mortality of the previous year. In 1819, the rate again increased to 1 in 13; and though this took place during the operation of the dry system, it is satisfactorily shown by Dr Daniell, that it did not, as was erroneously asserted by the opponents of the measure, arise from it. It appears that to the permanent population 1819 was a healthy season. This, however, had been enlarged by the accession of a numerous body of labouring whites, who, arriving from Europe in the Savannah during the months of July, August, and September, became easy victims to the malignant air of a climate to which they were as yet unseasoned.

The year 1820 was distinguished by a still more alarming increase in the rate of mortality in Savannah, the proportion of persons cut off by autumnal diseases having now amounted to 1 in $5\frac{1}{10}$; and this calamitous reverse was singularly unfavourable to the popularity of the dry culture system, which was now confidently charged, not only with being quite inefficient to prevent disease, but as itself a prolific cause. Independent, however, of the contradiction afforded by subsequent experience, this preposterous notion is amply refuted by Dr Daniell, who has justly pointed out the true causes of this increased sickness. Previous to the winter of 1818, 1819, the labouring drudgery was performed chiefly by the coloured population; and white porters and draymen were unknown, at least in the city of Savannah. The great influx of white strangers during this season, and the consequent mortality among them in the autumn of 1819 has been already remarked. During the winter of 1819 and 1820, they continued to be poured into the city, and along with those who survived, augmented the ordinary population to such an extent, that every habitable house was crowded. The fire which took place in January 1820 deprived many of the slender accommodation which they had obtained, and forcing them in on their countrymen, rendered them so crowded, that "frequently two or three families were found in small houses of one story, with but two small rooms, 10 or 12 feet square." By this overcharge, cleanliness and ventilation, which were previously not attended to, were now entirely neglected; and aided, perhaps, by the influence of the local causes of insalubrity, tended directly in the proper season to generate disease in a severe form, and over a great extent. A strong proof of the correctness of this mode of explaining the origin of the fever in 1820 is found in the fact, that the disease made its appearance chiefly in the eastern and western extremities of the town, where these people mostly resided, while in the central southern part,

which had been recently built, and the population of which was not crowded, no case of fever appeared during the whole season. The same view is also corroborated by the fact stated by Dr Daniell, that the new population afforded nearly three-fourths of the whole number of deaths.

Notwithstanding these untoward results, and the opposition which they enabled the opposite party to make, the committee for the system by dry cultivation persevered steadily in their purpose, and received at length some less equivocal proofs of the beneficial influence of their proceedings. In 1821 the mortality from autumnal diseases was reduced to 1 in 37; in 1822 it was 1 in $33\frac{1}{3}$; and in 1823 it was 1 in $32\frac{1}{6}$, giving an average of 1 in every 34 persons, which is not greater than the mortality of European cities in ordinary circumstances of health. During the last two subsequent seasons of 1824 and 1825, the public health of Savannah continued to improve still more remarkably; and the latter year is stated by Dr Daniell to be equally salubrious with that of 1818. Upon the whole, the short experiment instituted by the municipal government of Savannah, demonstrates unequivocally, so far as it goes, the influence which the proximity of rice-lands, under long flooding, exercises on the health of cities and towns; and it is not to be doubted, that wherever it is practicable, the cultivation of rice in such situations ought not to be allowed to approach so near as to endanger the health of the inhabitants.

The diseases which originate from this cause, and which become the frequent sources of sickness and mortality, are entirely similar to those which prevail in humid marshy districts, along the banks of rivers, or on low sea coasts, during warm seasons, and in tropical or hot countries. From the season in which they are most general Dr Daniell denominates them *autumnal* fevers, comprehending under this appellation intermittent, remittent, continued, and yellow fevers. Ague of the simple or double tertian form is to be regarded as the natural and uniform product of the soil. During the autumnal season it is rarely wanting among the poor residing in the eastern or western extremities of the city; and its affinity with the local character of the district is demonstrated in the fact, that, while yellow fever attacks strangers and new settlers, mild intermittent prevails among the natives and old inhabitants of the district. It also deserves remark, that the inhabitants of the eastern and western extremities of the city, where the grounds are low and damp, as well as the white inhabitants of the rice plantations, have in general enlargement of the spleen, or of the liver, or both,—the usual concomitants or effects of habitual and repeated attacks of ague. (p. 50.) The remittents of

Savannah are distinguished almost constantly by violent exacerbations on the alternate odd days; or, if this be not the case, by one exacerbation in the morning, and another in the evening, observing in the former instance the double tertian type, and in the latter that of the semitertian of the first order of Galen, or the true semitertian of Hoffmann. Continued fevers, which were never common, are now becoming more rare.

Cases of that concentrated form denominated *yellow fever* may be observed sporadically almost every season; but this disease has been epidemic twice only in 1817 and 1820 for a long series of years. The causes of these attacks have been already in some degree adverted to. Though inherent in the soil and the physical circumstances of the country, they were doubtless enabled to operate more forcibly by having for their subjects a considerable addition to the European unseasoned population. The epidemic of 1820, in short, appears to have operated principally, if not entirely, upon the recent European population. In the beginning of 1820, the same season during which so many Europeans were exposed to the insalubrious air of Georgia, nearly 300 negroes recently from Africa were captured on the coast by Union vessels, and brought into Savannah. Though these persons remained in the town during the whole time of the epidemic, yet not one suffered from the disease, while many of the house servants are stated to have become its victims. Dr Daniell, in illustration of this relative difference of susceptibility remarks, that negroes habituated to the atmosphere of rice plantations, are in a very great degree exempt from autumnal diseases; that the native African is more so than his offspring, and the field negro more than the house-servant, whose habits approach to those of the white population; and that this insusceptibility is impaired only by translation to a colder climate. These facts observed on the etiology of yellow fever, together with the simultaneous appearance of ague and remittents, and the resemblance between the necrological changes in the organs, induce Dr Daniell to regard these diseases as different varieties only of the same morbid action.

Dr Daniell reasons strenuously against the inflammatory nature of autumnal and yellow fever; and contends that the appearances of vascularity, or red and black points and spots found in the mucous surface of the stomach and alimentary canal of persons cut off by these diseases, arise from blood in the capillary veins, and depend on the same causes as hemorrhages in fevers; and, reviving the old notion of debility, he conjoins it with that of congestion, and declaims rather in an indignant tone against the theory of inflammation and increased action. He contends strenuously, on the other hand, for an atony or

insensibility of the skin and its vessels; and this, connected with corresponding congestion in the internal organs, is, in his opinion, the principal pathological condition of fevers.

In consistence with his opinions on the nature of the autumnal fevers, the present author is very hostile to the antiphlogistic mode of treatment generally practised by British colonial and American physicians. Blood-letting he condemns in unqualified terms; and he is not much more indulgent to the practice of giving purgative medicines. It is against the plan of treating the fevers of hot climates by mercury administered in large quantities, however, that Dr Daniell directs the principal strength of his reasoning. His arguments may be reduced to three general heads.

First, he contends, mercurial action is neither superior to, nor can control febrile action. Salivation cannot be produced if the latter is going on, and it may be suddenly suspended by an attack of fever. "A man who had passed a considerable portion of the preceding eighteen months in the hospital with an extensive ulcer in the groin, was admitted in the fall of 1822 with the same affection. It had been treated as syphilitic, and he had been several times salivated. A wash of lime-water and calomel was directed by me, which in a very short time produced profuse salivation. In a few days after he was attacked with remittent fever, upon the supervention of which the symptoms of salivation wholly disappeared. The fever terminated in nine or ten days, upon which the salivation returned in its full force, with mercurial fetor, swelled salivary glands and tongue, inflamed gums, and furred tongue. Not one grain of mercury was administered to this patient after the supervention of fever; nor was there, at that time, another case of salivation in any of the wards."

When salivation does take place, therefore, Dr Daniell maintains that it shows the febrile action to be subsiding, and is a proof rather of the mildness of the disease than of its open efficiency in controlling it. The following observations contain rather a sarcastic censure of the administration of mercury in yellow fever. "The advocates for the controlling influence of mercury in autumnal fevers, claim that, where salivation follows the declension or termination of such disease, that declension or termination was the effect of what they pleased to term 'the silent influence of mercury upon the system.' The silent influence of this medicine is, I believe, however, more fully recorded in our bills of mortality than anywhere else. One of the apothecaries of this place, in 1820, informed me that he retailed to the physicians of the city during that season about thirty pounds of calomel."

Secondly, Dr Daniell maintains, that though salivation be established in some few cases, it is unable to control the malig-

nant action of the disease. Upon the authority of Dr Cutter, he mentions an instance in which salivation and black vomit occurred in the same patient, the former preceding the latter by thirty-six hours, and yet without preventing the fatal issue.

Thirdly, Mercury given to produce salivation inflicts a real injury on the patients labouring under fever. So long as the febrile action is violent the mercurial does not take place. The moment the former begins to abate the latter is established, and from this time contributes to retard convalescence. The violent action of the mineral, with the great loss of saliva, impairs the general strength, and the recovering powers of the alimentary canal. In some instances, even, persons under mercurial influence are attacked with coma, followed by death, which Dr Daniell thinks was solely caused by the operation of the mineral.

The method of treatment which Dr Daniell has found most successful, will appear singular to those who have been accustomed to the profuse evacuations recommended by Rush, Jackson, and others. Proceeding on the idea of atony of the skin, and congestion in the internal organs, he applies sinapisms repeatedly to different parts of the skin, as the belly, the legs, the fore-arms, &c. until the surface is reddened, when the internal distress in general abates. After some ground is gained by this application, energetically carried on according to circumstances, he administers Cayenne pepper, (*Capsicum annum*,) with the view of equalizing the circulation (118) removing the cold stage and chillness of the surface, and relieving determination to the brain. The operation of this remedy, he conceives, is aided by the use of Virginia snakeroot, (*Serpentaria Virginiana*,) in the form of infusion of the root cold, and bark, or its substitute, sulphate of quinine, and in mild intermittents and remittents, Fowler's arsenical solution. He mentions an interesting case in which a sailor, labouring under a double tertian, took half an ounce of arsenical solution at two doses, with the effect of producing active purging and profuse perspiration, followed by immediate and permanent cure of his fever.

It is not easy to say in what light this innovation, in the manner of treating a disease so formidable as remittent or yellow fever, must be viewed. Some of the arguments which Dr Daniell has advanced against the mercurial treatment might, with equal justice, be turned against the remedies used by him, most of which would be regarded, and would be proved to be inefficient in severe epidemics. In every proposal for improving the treatment of these diseases a very great difficulty is found, in the circumstance that measures which prove remedial in one epidemic seem to have no influence whatever in another. In this

dilemma, the course is to choose that which experience has found most generally successful, and to employ every remedy, not only with due regard to the existing symptoms, but with attentive observation of its manifest effects.

Whatever opinion, however, be formed of his mode of treatment, the small treatise of Dr Daniell possesses considerable merit, in giving a succinct and clear account of the medical topography of the city in which he resides.

ART. VIII.—*Practical Observations on the application of Lunar Caustic to Strictures in the Urethra; illustrated by cases proving the permanency of the cure by that treatment. To which are added some remarks on its use in Strictures of the Œsophagus.* By M. W. ANDREWS, Member of the Royal College of Surgeons, London. Second Edition, London, 1827, 8vo, pp. 249.

THE treatment of strictures of the urethra, by the application of lunar caustic to the contracted part of the canal, though introduced by the high authority of John Hunter and Sir Everard Home, has not experienced from the profession that universal and unqualified reception, which is usually given to a remedy of undoubted merit and invariable efficacy. Directly opposed from various quarters on different grounds, its failure in the hands of several who were abundantly inclined to give it a full and impartial trial, has prevented it from obtaining that general confidence which is indispensable to the ultimate success of any remedy. Even after being recommended by the example of a surgeon so celebrated as Sir Everard Home, the use of lunar caustic cannot be said to be a general practice; and by many surgeons the employment of the armed bougie is still cautiously avoided, or reluctantly adopted only when other means have failed to effect a cure.

The application of lunar caustic, however, to strictures of the urethra, is precisely like that of every other powerful remedial agent. It is neither essentially bad, nor invariably or necessarily good. It is neither requisite in all cases, nor ought it to be utterly excluded from practice. Under proper management, and in suitable cases, it may effect most beneficial results, which shall in vain be looked for from any other remedy; and Sir Everard Home himself has not attempted to conceal the fact, that, if unskilfully or awkwardly used, it may produce great if not irreparable mischief; and that there is a class of cases in which its use is always injurious. It is no exaggeration

to say, that the objections which have been urged against its use have originated either in entire ignorance of its effects, or in misapplication or mismanagement.

After the comprehensive and satisfactory information successively communicated in the three volumes of Sir Everard Home,* and the clear statements in the various writings of Mr Charles Bell,† scarcely any thing more was required, either in the way of further evidence of advantages, or more complete refutation of objections. The practical observations of Mr Andrews, however, may be regarded as a useful appendage to the work of Sir Everard, and as a short but highly important statement of the actual virtues of the armed bougie in curing stricture. The merit of novelty the author entirely disclaims; and the work, therefore, contains little, either on the pathology or on the treatment of the disease, that is not found elsewhere. His principal purpose is to obviate the objections which have been urged against the employment of lunar caustic, and to convince the profession not only of the harmlessness, but of the advantages of this mode of treatment. This task Mr Andrews has certainly performed in a plain and practical, but very satisfactory manner.

In his sixth chapter the surgical reader, who still entertains objections to the employment of the armed bougie, will find them attentively considered, and opposed by facts so strong, that no candid mind can well refuse assent. It was perhaps superfluous to notice the objections so strenuously advanced by Mr Benjamin Bell of this city in his *System of Surgery*; for, whatever might be the force of these objections thirty years ago, when Mr Bell wrote, and when the real effects of lunar caustic were imperfectly understood, it cannot be doubted, that they must be considered as completely answered by the facts since adduced by Sir Everard Home. Exclusive of the supposed injurious consequences, when the application was properly made, the more serious evil, naturally imagined to result from the accidental dropping off of the caustic, has been shown by experience to be exceedingly trifling.—(Home, Vol. i. Sect. v. p. 504.) Mr Andrews gives, on the authority of Professor Brande, an ingenious, and, there is reason to think, an accurate explanation of the comparative innocence of a small piece of nitrate of silver being left in the urethra. It is well known by those acquainted with the phenomena of chemical action,

* Practical observations on the treatment of Strictures in the Urethra. By Everard Home, Esq. Vol. I. Third edit. London, 1805. Vol. II. London, 1803. Vol. III. London, 1821.

† Letters concerning the Diseases of the Urethra. By Charles Bell, London, 1810.

that this salt undergoes decomposition the moment it comes in contact with any substance containing chlorine. No sooner, therefore, does the urine pass over the metallic nitrate, than its muriatic salts effect decomposition, and form the chloride of silver, which is insoluble, and therefore inert as to the animal tissues. Soon after, in most cases, it is expelled; but even when it is retained, it is obvious that the simple rule of causing the patient to make water, laid down by Sir Everard, disarms the substance of all power.

Among the objections urged by Mr Whately, one of the most formidable is that of the use of caustic being sometimes followed by false passages. This objection is not peculiar to the armed bougie. It may, and has often happened with the ordinary one; and the surgeon who would produce a false passage by the lunar caustic, is equally liable to do so by the ordinary instrument, and perhaps more so, if possible, by the caustic potass.

In the seventh chapter, on the comparative advantages of the armed bougie, Mr Andrews is at pains to show, that this instrument, when managed properly, and with suitable precautions, is not only free from all danger, but is perfectly adequate to effect with safety a complete and permanent cure in the most severe, complicated, and obstinate cases of stricture. He deviates a little from the practice of Sir Everard Home, in touching the stricture slightly, and withdrawing the bougie immediately, and in never keeping the caustic in contact with the strictured part longer than a few seconds. Though this cautious mode of procedure renders it requisite to repeat the application oftener than according to the method of Sir Everard, yet this inconvenience is, in the estimation of Mr Andrews, counterbalanced by the greater security which, he conceives, it affords against the occurrence of hemorrhage.

The author illustrates his doctrines, and corroborates his reasonings by several apposite cases interspersed throughout the volume; and the eighth chapter consists of a series of forty-nine cases of various sorts of stricture, and other forms of urethral disease, permanently removed under the use of the armed bougie; thus affording the strongest possible evidence of the beneficial effects of this mode of treatment.

In the ninth and concluding chapter Mr Andrews relates the particulars of three instances of stricture of the œsophagus in which he adopted the same method of affording relief. Two of them went on to the fatal termination; and though in one the stricture was found after death to have been much reduced by the use of the caustic, in the other, in which, from particular circumstances, its free application was deemed inexpedient, the

strictured portion was but little improved. In the third case, six applications of the caustic was followed by considerable relief, which appears to have been permanent.

ART. IX.—*On Medical Education.* (Continued from last Number, p. 377.)

WHEN the former part of this article was prepared for publication, the author was fully aware of the delicacy of the task he had undertaken. He had to come into collision with the opinions or interests of several individuals, and of public bodies, with all of whom he stood in various relations of connexion. He took the step, unprecedented in the Journal, of subscribing his initials to the article, to take upon himself the whole responsibility of every statement, and of all the arguments. About the light in which the latter might be viewed, he was comparatively indifferent, as the readers were capable of judging for themselves; but in regard to the former he was exceedingly anxious, and took every pains to be accurate, not only from the love of truth, but because he knew well that even the slightest inaccuracy would not escape detection and exposure.

As the author anticipated, the article has not passed without animadversion. In the report of the proceedings of the Town Council of March 28, published in the Edinburgh newspapers, it appeared that it had been noticed and commented upon at the board of that body, whose decisions on the points at issue were of so much importance to the university and the country. The author, desirous to be corrected in any error or omission in his statements, thought it incumbent on him, however reluctantly, to address the following letter to the gentleman by whom the remarks were said to have been made.

Dr DAVID HAY, Surgeon,

MY DEAR SIR,

I see by the report of the Town Council proceedings in the Courant Newspaper of Thursday last, that you took notice of an article on Medical Education in the last number of the Edinburgh Medical and Surgical Journal, which was written by me.

As in that article I endeavoured to be as accurate in the statement of facts as possible, and as you have represented my statement, in regard to the dispute and law-suit between the Patrons and Senatus, as being essentially defective, I beg you to do me the favour to point out those deficiencies as to matters of fact which you consider as essential, in order that I may either correct my statement in the continuation of the article in the next number, or else

insert your objections to it with such remarks thereon as may occur to me.—Yours most truly,

ANDREW DUNCAN JUNR.

45, York Place, 30th March 1827.

To this letter the following answer was next day received, which, in conformity to promise, is sent for publication, with a few notes chiefly upon the matters of fact.

MY DEAR SIR,

7, Queen Street, 31st March 1827.

As the report in the *Courant* of Thursday does not contain the whole of my statement to the Council, I think it due both to you and to myself to afford you a copy of what I actually stated on the occasion. “I begged leave to call the attention of the Council to an extract from the number of the *Edinburgh Medical and Surgical Journal* now published, and which I believed had been sent to all the members.

“This extract professed to be a review of certain essays on the subject of medical education, but commenced with what is called a short historical account of the circumstances connected with the disputes betwixt the Patrons and the Professors of the University regarding their respective rights.

“This statement, or historical account, I conceived to be uncalled for and indelicate at the present time, whilst the discussion was pending in a court of law; but, lest it should influence the opinions of those members of Council who had lately joined that body, I recommended them to consult the documents and correspondence in possession of the Council, from which they would see that the statement given was essentially defective, and calculated to produce an erroneous impression.

“I felt satisfied that the Council, as Patrons of the University, had acted from no other motive than the wish to promote the public good, and the welfare of the medical school of Edinburgh; and I should be much deceived indeed if the Royal Commission, after the accurate and minute investigation they had commenced was completed, did not give them full credit for their anxious endeavours to advance the interest of the university of which they are the Patrons.

“With regard to the other subjects contained in the review, I did not presume to offer any remarks, being fully assured that the Royal Commission would judge impartially of, and give effect to that course of education which, from the evidence they received, appeared to them the best fitted for the learned professions, without looking to any other circumstances than those which regard the respectability of these professions and the good of the community.”

These are nearly the *ipsissima verba* which I made use of; and I certainly should not have made any statements at all, had your paper not been circulated amongst the members of the Council. I did not attempt to influence the opinion of any individual, but considered it my duty, as having been a member of the Council when

the unhappy disputes were proceeding, to call on the younger members of the Council to form their own opinions by consulting the documents in their power.

I conceive any discussion or correspondence on this subject betwixt the Council, or individual members of it, and the professors, to be as improper as it is unnecessary at the present time, and therefore trust that you will excuse my wish to decline your request.— I have the honour to be, my dear Sir,

Your most obedient servant,

To Dr DUNCAN JUNR.

DAVID HAY.

We shall now make a few observations in reply to this letter ; 1st, In regard to the reasons stated for taking notice of the preceding part of this article in a meeting of the Town Council ; and, 2dly, In regard to the reasons given for declining to point out the asserted deficiencies in our statement.

The article is said to have been noticed only because copies of it had been sent to the members of the Town Council, and in order to caution the younger members against being misled by a statement essentially defective, and calculated to produce an erroneous impression, and to call upon them to form their own opinions, by consulting the documents in their power. In reply, it may be stated that it was an act of respect, if not of indispensable justice, to the members of the Town Council, to transmit to them copies of an article, in the effect of which, however slight, on the public mind, they are much concerned ; and that it was not incumbent on us, even if we thought our own article defective, to suppose that any member of Council could be misled by a statement inconsistent with documents in their possession, or that they would have failed in their duty so far as not to have made themselves acquainted with the history of an important proceeding carrying on in their name, and by their authority. Nor can we avoid remarking, that the want of information here admitted on the part of those members of Council who had recently joined that body, is a strong confirmation of the arguments by which we endeavoured to show that a fluctuating body, such as the Town Council, is not qualified to regulate those details of university discipline, which ought to be of great durability, and which, in every similar institution, belongs exclusively to the senate, or some permanent authority.

In reference to the allegation that our statement is defective, it happens, rather remarkably, that the (official ?) report of the very speech, not a long one, in which this allegation is made, is stated by its author to be essentially defective. It is evident that every statement which attempts to condense in a few pages, what at full length would occupy many sheets, might be easily enlarged ; but after carefully reconsidering our statement, we are

still at a loss to understand wherein the alleged essential defect lies. Anxious, however, to correct any error into which we might have unintentionally fallen, we did not hesitate to solicit a communication of the individual points upon which the general charge was founded; but our request was declined for reasons which appear to us totally unsatisfactory.

We were told that any discussion or correspondence betwixt the Council, or individual members of it, and the professors, would be as improper as it is unnecessary at the present time; and the Council were even informed that our statement, or historical account, was uncalled for and indelicate at the present time, whilst the discussion was pending in a court of law.

We shall not cavil at the use of the term *Professors*, although our statement was written in the capacity of *Reviewers*, and contained nothing that any person, not a Professor, might not have known; but we maintain that we were entitled, in common justice, to demand a communication of the specific grounds upon which such a general charge was made against us, first in the Town Council, and then at the bar of the public.

In making such a charge all considerations of indelicacy and impropriety were completely set aside, and cannot be afterwards resorted to as a defence against the consequences of such a step. But we assert that our statement was rendered absolutely necessary to put a stop to the misrepresentations industriously propagated by persons apparently not very well-disposed to the university, or to the medical faculty in particular. Our statement was public, and for its accuracy we made ourselves responsible.

As to the indelicacy of giving accurate information to the public concerning the origin of the discussion between the Patrons and Senatus, whilst it was pending in a court of law, we have to state, that we carefully avoided making a single remark on the legal questions at issue, that imperfect accounts had been previously published, and that the public, distracted by conflicting rumours, was entitled to an authenticated statement. The history of the transactions which led to the trial of the legal question of the power of the Town Council over the Academical Regulations interests the medical profession generally; and it interests the medical professors more especially, (whatever be the result of the suit at law,) to show that they were not unmindful of the character of the university, or the credit of its degree, and that it was not on light grounds, nor without due consideration of the consequences, that they, in concurrence with all of their colleagues, (the professor of midwifery excepted,) determined on resisting the demand of the patrons. But at all events, if there were any indelicacy in the matter it does not attach to us in the smallest degree.

The first communication to the public of the unfortunate differences between the Senatus and their Honourable Patrons was transmitted from the Town Council, whether officially or not, we cannot say. It appeared originally in the Scotsman, and was copied by the other Edinburgh Newspapers.

“ Town Council.—At an extraordinary meeting of the Council held yesterday, the opinions of the Dean of Faculty and Solicitor General were read, as to *two points long in dispute between the Council and the Senatus Academicus*. These two lawyers were of opinion, that *the power of raising the students' fees, claimed by the Senatus*, belongs exclusively to the Council, and also *the power of regulating what professorships shall belong to the various faculties*. This latter question arose out of the recent annexation of Dr Hamilton's chair to the Faculty of Medicine. Some discussion followed; but it was agreed to let the matter lie over for consideration till this day.”—Scotsman, September 14, 1825.

As this paragraph was calculated to mislead the public, it was immediately met by the following counterstatement, the accuracy of which has not been questioned, and is unquestionable. We have printed the discrepancies in italics.

“ University of Edinburgh.—We are requested to state that the account of the proceedings at an extraordinary meeting of the Town Council, in reference to the Senatus Academicus, which appeared in our paper on Thursday last, is altogether erroneous. That statement was copied from the Scotsman; and we have authority to say, that neither of the two points said to have been long in dispute between the Council and the Senatus Academicus has been under discussion between those bodies; and no mention is made of the only point which really has been under discussion, viz. *Whether the course of study necessary for becoming a candidate for a degree shall be regulated by the Senatus Academicus, as in all other universities, and hitherto in our own, or by the Town Council.*”—Edinburgh Evening Courant, September 17, 1825.

The Scotsman, to which a similar communication was also sent, inserted its substance, and very handsomely added a request, that the papers which copied from them the former notice would copy the corrected statement, thus admitting that their former statement was not correct.

It may be said that this first communication was previous to the commencement of the law proceedings; but this defence will not avail; for, after the summons against the Principal and Professors was issued at the instance of the Honourable Patrons, partial statements concerning it continued from time to time to be inserted in the newspapers, under the title of Town Council Proceedings, which, from the deliberations of that body taking place with shut doors, could be communicated only by one of

themselves, or authorized by them. The charge of indelicacy and impropriety recoils, then, from us upon some individual connected with the Town Council; and it is with peculiarly bad grace that our taking the same mode of defending ourselves should be censured in that body which allowed the repeated aggression by one of themselves to pass unnoticed.

The truth is, that the transactions in question have been matter of pretty general public discussion, though often misrepresented, for some time past, both before and since the dispute was brought into the courts of law. Dr Hamilton published and circulated various papers connected with them; and Dr Thomson, in his first pamphlet on Medical Education, not only alluded to the dispute then in existence, (though not yet before the courts,) but actually anticipated its decision against the university; addressing himself to the Patrons, as well as the Professors, although the interference of the former in such matters was known to be unprecedented; and the same title-page was prefixed to his Additional Hints, published after the legal proceedings were far advanced, and the Royal Commission in existence. Dr Clark, also, pending the law-suit, uses the same address. We are not aware that any of these proceedings were complained of as indelicate or improper; but, as Sancho says, "it is safer for one man to steal a horse than for another to look over a hedge."

The Professor of Clinical Surgery in this university has also honoured us with his notice.* He objects to our having said that the President of the Royal College of Surgeons is at the head of another, and in some respects a rival school of professional education. But his whole argument proceeds upon a misconception, as if we had represented the Royal College of Surgeons as a *hostile* school. On the contrary, we consider that the fair competition existing between the two schools is advantageous to both; but we must repeat, that they are in some respects *rival* schools. They both grant professional diplomas, and the same curriculum of study is not required by both.

We now proceed with the other subjects noticed in the pamphlets under review.

Dr Thomson suggests, as an improvement on the constitution of the Faculty in the University, strictly medical, Whether "the duties at present performed by the Professor of the Theory of Medicine should not be assigned to two distinct professors at least—Physiology to the one, and Pathology to another?" The

* Remarks concerning the Professorship of Surgery and Private Lecturers, addressed to the President and Fellows of the Royal College of Surgeons of Edinburgh. By James Russell, F. R. C. S. E. Professor of Clinical Surgery. Pp. 14. 8vo, Edinburgh, 1827.

Professor of Midwifery takes a very different view of this subject of medical instruction ; for in his famous Memorial to the Honourable Patrons, he broadly asserts, that “the students can derive from that individual Professor no additional knowledge which may enable them to cure disease.” Who shall decide when doctors disagree ? On this occasion at least, we may say *Medio tutissimus ibis*. Leave well alone.

The next subject discussed in the pamphlets whose titles are prefixed to this article, which requires any notice, is the proposal to institute a Professorship of Surgery distinct from that of Anatomy. This has long been our own opinion ; and, as a mere matter of expediency, we should recommend their separation, if the consent of the present professor of anatomy and surgery could be obtained ; for we cannot suppose that the Honourable Patrons, or even the Royal Commissioners, would think of interfering with the rights of the present Professor of anatomy and surgery. Notwithstanding our determination, in considering the various reforms upon medical education suggested in these pamphlets, to leave all legal considerations out of view, and to consider the questions abstractly, we cannot avoid saying, in opposition to Dr Thomson’s long argument, in which he is so tenacious of very doubtful corporation rights, that, from the terms of his commission, the right of Dr Monro to be professor of surgery so long as he lives, appears to us to be as complete and irrevocable as that of the professor of any other branch of knowledge in the university.

It would, however, be unjust to Dr Monro, if, in concurring with Dr Thomson as to the expediency of the measure, we took advantage of the mistake into which the latter has somehow fallen, in supporting his views on the ground of precedent. He doubts whether there will be found on the continent of Europe a single instance of an university containing a medical school, in which one professor is charged with the duties of teaching both anatomy and surgery ; and he cites the universities of Bologna, Breslau, Bonn, and Berlin ; of Freiburg, Giessen, Greifswald, Gottingen, and Geneva ; of Halle and Heidelberg ; of Leipzig, Marburg, Moscow, and Munich ; of Paris, Padua, Pavia, and Pisa ; of Rome, Turin, Vienna, and Wurzburg, as places where anatomy and surgery are taught by separate professors. In regard to most of these Dr Thomson is correct, but as to others we know that he is mistaken. We have not time to consult the programmes of all the continental universities, but in those at hand we find the following exceptions to Dr Thomson’s statement. At Gottingen, “ C. J. M. Langenbeck, Dr. *Anat. et Chirurg. Prof. hora i. ii. demonstrationes anatomicas, in quibus*

splanchnologia, angiologia et neurologia pertractabuntur ex libro suo (anatomisches Handbuch tabellarisch entworfen) et iconibus suis anatomicis subsidiis instituet; hora ii. iv. in secundis cadaveribus auditores instituet; hora vi. vii. P.M. chirurgiæ alteram partem tradet; hora ix. x. exercitationes clinicas in nosocomio chirurgico diriget. Privatissime instituet, 1. Cursum chirurgico-manualem; 2. exercitationes operationum circa oculorum morbos suscipiendarum." At Rostock, Josephi teaches both anatomy and surgery; at Upsal, Adolphus Murray is professor of anatomy and surgery; in Greifswald, Rosenthal teaches anatomy and surgery; at Heidleberg, Fohmann. We also recollect that at Pavia Scarpa taught anatomy and clinical surgery, and he was previously professor of anatomy and surgery at Modena.

As soon as a distinct professorship of the principles and practice of surgery shall be established, the chair of Military Surgery will be rendered superfluous, and should be absorbed into the other. In fact, the peculiar province of this chair has no more connexion with surgery than with medicine. In Berlin, it is entitled Military Medicine, and sometimes it has been called Military Hygiene. It is also understood to embrace the professional care of seamen as well as of soldiers. As the most practicable method of immediately extending the surgical instruction given in the university, we suggest, for the consideration of those concerned, that a new commission should be issued, nominating the present incumbents of these chairs joint professors of the principles and practice of surgery, with such reservations for supplying vacancies as may be thought most advantageous for the interest of the public.

The last branch of medical education discussed in the pamphlets upon which we shall make any remarks, is Clinical instruction, perhaps the most important of all.

The ultimate end of our varied study is to qualify us to practise medicine as a profession. In all probability, the first mode of instruction was clinical. A father would naturally carry his son along with him, as soon as he was old enough, to visit his patients, and to assist him, and would give him instructions to enable him to act as a substitute in his absence. Hence, among the Egyptians and other nations who retained their primitive customs, medicine, as well as other professions, became hereditary, by custom or legal enactment; and even to this day, from this natural cause, we see hereditary teachers as well as hereditary practitioners of medicine.

On other occasions, the assistance of servants would become necessary, and on the credit of having acquired some of his

knowledge and skill, on his decease, or when they found a fitting opportunity, they would set up for themselves, as the pupils of their master.

The progress from this stolen instruction to apprenticeship is easy and natural. The master would soon find it more for his interest to form a contract with his pupil, by which he would secure to himself his assistance for a considerable period of years, and in return would engage to instruct him as far as his opportunities permitted him. Accordingly, apprenticeship, under various appellations, became the general method of instruction in all the arts, and even some of the sciences, until the erection of the universities in various parts of Europe, in which might be received a liberal education, or an education without servitude. But in the universities an opposite fault was committed, for the instruction given in them erred in not being sufficiently practical. It was believed that great learning and profound reflection were sufficient for the acquisition of knowledge; reasoning *a priori* was chiefly cultivated: and the conclusions of subtle minds were impressed dogmatically on their pupils, as the doctrines of the schools. With the progress of the inductive philosophy, however, scholastic learning gradually became less esteemed and now scarcely maintains a feeble existence, supported by foundations whose rules were fixed in less happy times.

The method of teaching medicine by clinical attendance seems to have been revived systematically by the monks, who were the original medical officers of the hospitals in the south of Europe. These charitable persons were attended in their visits to the sick by their younger brethren, to whom they gave instruction, and from whom they received assistance; but the first regular clinical lectures were probably given in Padua in 1578, and about a century thereafter, clinical instruction was established in the medical schools of Holland.—Dr Monro *Primus* attended the clinical lectures of Boerhaave in 1718, in imitation of which those of Edinburgh were instituted.*

Before the organization of the Medical School of Edinburgh,

* It is remarkable that John Peter Frank, in his extensive work on Medical Police, (*System einer vollständigen medicinischen Polizey*, 6ter Band, 1 Thiel, 8vo, Wien, 1817,) should have conferred upon Edinburgh the honour of introducing the teaching of clinical medicine. Although not strictly true, this statement by one who has written on the best arrangement of clinical schools, (J. P. Frank, *Plan d'Ecole Clinique*, Vien, 1790,) shows the high estimation in which the clinical school of Edinburgh was very early held by the best informed on the continent:—"Until the eighteenth century the practice of the healing art, after the theoretical studies were completed, was to be learned by young physicians left to themselves, in the most laborious manner, and with the sacrifice of hundreds of patients; but this evil has certainly been greatly diminished by the practical instruction communicated to young physicians, by experienced practitioners at the bedsides of the patients, *first in Edinburgh*, and then in Leyden and Pavia, and soon afterwards in other places."

an inhabitant of this kingdom was obliged to resort to foreign countries to receive a liberal education ; and the Royal College of Physicians of London was formerly composed chiefly of foreign graduates, though now restricted to those promoted in the English universities.

In as few words as possible, we shall give the history of the origin and rise of the medical school of this city.

Although a professorship of botany was founded in 1685, and the appointment conferred upon Sir Robert Sibbald, Bart. who has thus the honour of being the first professor of medicine in the University of Edinburgh,—a professorship of anatomy in 1705, to which Mr Elliot was nominated,—and a professorship of chemistry in 1713, which Mr James Crawford first enjoyed, nevertheless the date of the origin of the Medical School of Edinburgh may be fixed at 9th February 1726. At that time the Honourable Patrons of the University granted a commission to Andrew Sinclair and John Rutherford, doctors of medicine, to be professors of the theory and practice of medicine, and to Andrew Plummer and John Innes, doctors of medicine, to be professors of medicine and chemistry in the College of Edinburgh, with full power to all of them to profess and teach medicine in all its branches, as fully and freely as the said science is taught in any university or college in any country ; and bestowed upon the said professors all the privileges, liberties, and immunities that were, or afterwards might be, enjoyed by the professors of any other science, and, particularly, with full power to them to examine candidates, and to do every other thing requisite and necessary to the graduation of doctor of medicine.—The professors so appointed were afterwards recognized by the Senatus as the Medical Faculty.

On the same day they nominated Mr Joseph Gibson as professor of midwifery in the city and privileges, but he does not seem to have been a member of the Senatus Academicus ; neither at that time were the professors of botany and of anatomy. The professor of botany, however, became a member in 1738, when Dr Alston received a commission as professor of medicine and botany, and the professor of anatomy in 1756, when the Monros *Primus* and *Secundus* were admitted. Until 1767 the *Materia Medica* was taught by the professor of botany ; but on Dr John Hope's resignation of the former department, *materia medica* was erected into a separate chair, which was first held by Dr Francis Home.

The Royal Infirmary was founded in 1738 ; and when George Drummond became Lord Provost in 1746, soon after the battle of Culloden had restored confidence in the stability

of the government, one of his first acts was to institute clinical lectures in the Infirmary.

The interest of this institution was evidently interwoven with that of the university. The managers "resolved to adopt every measure that could tend to facilitate medical education, and to render it complete. They therefore permitted students of medicine, upon paying a small gratuity, to attend the hospital, that they might have all the benefit that could be derived from the practice of the physicians and surgeons. What the students paid for their admittance was allotted to the annual support of the house, and hath now arisen to a considerable sum. Farther, the managers, considering that the defect of clinical lectures in medical seminaries had often proved a ground of complaint, gave liberty to the professors of medicine to lecture on such cases of the patients as they should find most conducive to the instruction of the students."—(History of Royal Infirmary, p. 16.)

Dr John Rutherford, Professor of the Practice of Medicine, had the honour of delivering the first course of clinical lectures, in the winter session of 1746–47, which were attended by a great many students, (Scots Magazine, xii. p. 52,) and since that time they have been given uninterruptedly and regularly. Until about 1756 Dr Rutherford seems to have been the only professor who gave clinical lectures; but at that time his bad state of health rendered assistance necessary, and Dr Monro *Primus*, Dr Whytt, and Dr Cullen, undertook to supply his place; and as Bower, in his History of the University, rightly observes, perhaps no hospital was ever so appointed.

The clinical school of Edinburgh did not rise to eminence by slow degrees; but almost on its first institution became the most celebrated in Europe. Nor can we be surprised at the reputation it so rapidly acquired, when we recapitulate the names of those who have preceded the present clinical teachers in this charge, viz. Dr John Rutherford, Dr Monro *Primus*, Dr Whytt, Dr Cullen, Dr John Gregory, Dr Francis Home, Dr James Gregory, Dr Daniel Rutherford, Dr Andrew Duncan Sen.

Under the direction of these able teachers the clinical school of Edinburgh was for a long period esteemed as one of the first in the world, not only by those whose natural partiality might lead them to exaggerate its advantages, but also by dispassionate foreigners in works deliberately written upon the best modes of communicating medical instruction, or in Polemical Discourses, in which established institutions were subjected to a severe comparison with ideal perfection.

It is therefore not without some surprise, nor even without some feelings of indignation, that all at once we find some who might be supposed to be acquainted with its present state

representing it as behind the progress of the times, when compared with the clinical instruction in other universities, and as standing in need, not merely of reform, but of utter subversion, in order that another system may be substituted in its place.

The faults found with the present system by Dr Thomson and Dr Clark do not altogether coincide, and their plans of reform are likewise different. Dr Thomson would deprive the present members of the medical faculty of all connexion with the teaching of clinical medicine, and would confide it entirely to one individual nominated to that duty; while Dr Clark would leave to certain members of the medical faculty the present hospital clinic, with some modifications in its detail, but would require from medical graduates attendance on clinical surgery and a course of practical clinics.

By summing up what he considers as the defects of the present system of teaching clinical medicine in six propositions, Dr Clark will save us some trouble; and we now proceed to comment upon them *seriatim*.

His first objection is, that "the period of attendance required of the candidates for a medical degree is not sufficiently long to admit of his acquiring such a degree of practical knowledge as every graduate ought to possess." This defect Dr Clark proposes to remedy by extending the period of clinical instruction to three courses; the first to be entirely devoted to clinical surgery, the second to the hospital clinic, which is at present given at Edinburgh; and the third to the practical clinic. To this extension of clinical study, the chief and almost only objection is the increased time and expence which it would require the student to devote to his strictly academical studies. The extension of the period of the study of clinical medicine was at one time contemplated, and almost determined upon; but after much deliberation, it was relinquished, in order to leave to the students time and money to attend to other branches of academical instruction, and to prosecute their studies partly in hospitals, dispensaries, and other institutions not connected with universities.

In considering the amount of study required to obtain a professional degree, the distinction between compulsory and optional studies is not sufficiently attended to. The compulsory studies are, from a principle of liberality, reduced to the smallest amount possible; so that alone, they are, if at all, very rarely sufficient to complete a professional education. Optional studies, to a considerable amount, are followed by almost all of the students; and these they are left to take either from the professors, or private teachers, or by apprenticeship, or in places where there is no university. The very strict examination the

students undergo is in practice found to afford sufficient security for their course being rendered complete by optional studies.

The modes of regulating the studies for acquiring the highest degrees in medicine may be reduced to three. First, the completely compulsory, by which not only the total amount and duration of study necessary, but even the order in which it is to be taken, are fixed by enactment, as is the practice in Austria, France, Italy, and some other despotic states. Second, The absolutely free, which requires no compulsory studies of any kind, but leaves the information, the possession of which is to be ascertained by strict examination, to be acquired by the candidate in what manner, or order, in what time, or where he pleases. This was the case in Leyden, when Holland was a free country, and is still the case in the protestant Universities of the north of Germany. The third method is intermediate, when a certain period and amount of academical study are required. This is the practice of the Scottish Universities, of Dublin, and, we believe, of those of the United States; and we are inclined to think it the best. On this point we must be permitted to express our surprise at the recommendation that the regulations of the despotic governments on the continent should be introduced into this free country, and that we should be invited to take, in matters of education, the kingdom of the Netherlands, or of Sardinia, as a model. In such countries long and extensive courses of study, which cost the pupil little, and a certain fixed arrangement of the order in which each subject shall be studied, whether it be most for the advantage of the pupil or not, may be enforced without great injustice, as these regulations affect all alike who are to live by the same profession; and the practitioners, when licensed by the proper authorities, are protected in a monopoly of their profession; but the introduction of any such plan into our schools would be to retrograde violently in one of the most important objects of national policy, and to substitute in education, a subject which perhaps bears foreign interference less than any other, a restrictive for the present liberal system which distinguishes the policy of Great Britain from that of other European countries. What would our farmers think, if the Roman regulations in regard to the management of their lands were proposed to be adopted; if they were to be prohibited from sowing what grain they pleased, from irrigating one meadow, and even obliged to direct the waters upon another; to leave these fields fallow, and to plant those with vines according to positive regulations, and the judgment of government inspectors? They would treat the proposal with contempt, and would have no difficulty in convincing every person of common sense, that the interests of the whole community

would be best consulted, by leaving every one to use his own judgment in promoting his own interest. But the same liberal system is still more necessary in regard to education. Each individual is, upon the whole, the best judge of what he ought to study, and where he can study it to most advantage, with a view to the demands of the public, for which he is preparing himself, and to the competition which he has to encounter. We should have been less surprised had an opposite line of argument been taken, and universities and degrees declaimed against, as impeding the progress of knowledge, by an unjust monopoly and ridiculous titles; and indeed this argument is inconsistently pressed into the service on one occasion, when it is said "the members of the medical faculty seem hitherto to have forgotten, that it is the possession of a sufficient stock of preparatory and of professional knowledge, on the part of those who receive degrees and diplomas, in which the public is interested, and of very little consequence to it, in what schools this knowledge has been obtained."—Hints, p. 20.

The introduction of a compulsory uniform course of study would be attended with greater evils in Edinburgh than in almost any other school. It would almost destroy, or at least greatly lessen that competition with the many excellent private teachers, by which the exertion of the professors is secured under the present liberal system. A compulsory uniform course of study, like those in the despotic countries of the continent, would be necessarily complete within the University; a repetition of attendance on many of the essential branches would be enjoined, as in Paris, where anatomy, physiology, external pathology, internal pathology, clinical medicine, and clinical surgery, are each taken twice, leaving the student no time or occasion to attend non-academical lecturers, whom the pupil might prefer as furnishing the desired instruction either at a cheaper rate, or in a manner more suitable to his wants, or even as presenting a different view from the professors; whereas the Edinburgh Senate, by not requiring any class to be repeated in Universities, leaves the student at liberty to repeat the important branches, as they uniformly do more or less frequently, either in or out of the Universities, as may appear most to his advantage. A compulsory uniform course of study is particularly inapplicable to students, of whom a large proportion, as at Edinburgh, have already made some, and many considerable progress in their professional study, before they enter at the University. The extent of the monopoly exercised by the University is, that every candidate is required to attend in it, or any other school where the medical degree is given, each of the necessary classes once. Many are advanced in their studies be-

fore their university attendance begins, and to them it is often most useful to renew their acquaintance with the elementary branches, by attending to them in the last year instead of the first.

Another great objection to a compulsory fixed course of study is, that it would greatly interfere with the free and useful competition which at present exists among different Universities, foreign as well as domestic. Many of those who at present obtain the degree of M. D. at Edinburgh have received part of their education at other schools, as Glasgow, Dublin, or Paris, and they arrange their studies so as to attend those branches in each place for which they are most celebrated; but under the compulsory system, wherever they were, they would be compelled to attend all the branches set down as the studies of that particular period of their progress, however much a different arrangement might be to their advantage. Upon this subject we take the liberty to extract a few sentences from a letter we have recently received from the conductor of one of the most esteemed medical journals of Germany, to whom we had written for information on some points about which we were not absolutely certain. “Until some years ago, there existed in all the Universities of Germany, (with the exception of Austria alone,) the indiscriminate liberty to the students to hear what lectures they wished, with whom, in what order, and even not at all, provided always they were able to bear an examination in presenting themselves for a degree from the faculty, at whatever time suited them. This liberty has been a little encroached on during the last ten or fifteen years. Students of medicine are obliged to prove a *triennium medicum*, &c. but all these limitations are enforced only on the natives of the different states of Germany, (Prussia, Saxony, Hanover, Hesse, Holstein, Mecklenburgh, Bavaria, Wirtemberg, &c.) Those who are not natives of those principalities, are in the same boundless state of liberty as formerly. Some of these states, though not all, have ordered that the students, wishing to be employed in public offices, must pass one or two years of the triennium in their own University, for the purpose of getting acquainted with the laws of the country, &c. Those of the students who prefer to hear no lectures at all, or those of private teachers, (though the greatest talents and means of instruction are found among the regular professors,) may do so; but they will run the risk of being more rigidly examined by the professors composing that faculty,”—“In Austria, the time and order of studies are fixed by law. The result is, that there has been a more scanty and less thorough information obtained, than in the rest of Germany. With us some have

learned nothing at all, but some have reached the highest pitch of instruction. In Austria, all know a little, but nobody is eminent in the profession."

The same opinion is strongly expressed by a philosophical author who has written much upon the constitution of universities. "In Germany," says Meiners,* "even to the present day, the Catholic and Protestant universities are diametrically opposed to each other in most points. In the Catholic universities all, or at least the most important, lectures are public; while, on the contrary, in the Protestant universities, fees are paid by the student for permission to attend them. In the former, the duration, commencement, end, and hours of teaching, as well as the systems, mode of teaching, text-books, and course, which the teachers and the taught are to pursue, are fixed by law. In the Protestant universities, the professors and pupils enjoy as yet an almost unlimited freedom. In the Catholic universities the object is to compel the masters and pupils to do what is thought good for them; in the Protestant it is rather to stimulate them to do well. In the Catholic universities all competition of teachers is purposely prevented; in the Protestant, it is encouraged even beyond what is expedient. In the Catholic universities the lectures are more commonly for the whole year than for six months; in the Protestant the reverse. Lastly, in the Catholic, the lectures are delivered only, or at least most frequently, in public halls; in the Protestant, in rooms in the houses of the individual professors."

Again, "compulsory plans of academic study continue the errors, which it would be desirable to avoid in the lower schools, into the higher. It is unfortunate to be obliged to treat boys, who are very unequal in natural and acquired powers, on the same footing. But it is much worse when this is attempted with young men who differ from each other much more than boys. Plans of study, which are prescribed as in Mayence and the hereditary states of Austria, compel the pupils possessed of genius, knowledge, and industry, to proceed at the same rate as the stupid, ignorant, and idle, and by this very means the happiest dispositions in young men of the greatest promise are destroyed. The contrivers of these plans of study had almost, without exception, the good intention of conferring upon the scholars a very perfect and well-grounded education. They therefore comprised in these plans many sciences which a great many of the pupils either had really no occasion for, or at least considered as superfluous, or for which they had neither inclina-

* Cp. Meiners Ueber die Verfassung und Verwaltung deutscher Universitäten, 2 Th. 8vo, Gott. 1801-2.

tion nor abilities." We think we have said enough of compulsory uniform courses of study !

The second objection is, that "one or even two professors are unable to teach clinical medicine efficiently to the number of medical pupils at present frequenting the University of Edinburgh." As a remedy for this evil, Dr Clark proposes that the number of clinical professors in office at a time should be increased.

The present clinical professors deserve at least the credit of having endeavoured to obviate this objection by doubling their labour, without any additional expence to the student, and greatly to his advantage, by two professors instead of one attending daily, and thus dividing the concourse of students in the wards while all are admitted to the lectures of both. It is not in their power to go further, as they are under the control of the Managers of the Infirmary, and cannot exclude from the clinical wards the gentlemen who have hospital tickets, and are not clinical pupils. These very generally prefer attending the clinical teachers to going round with the ordinary physicians. This preference of the clinical professors to the ordinary physicians, who are not inferior either in skill or in regularity of attendance, is the best proof that the students find advantages which more than counterbalance any inconvenience arising from number. At any rate, we fear, that, if all the plans of these reformers were to be carried into effect, this objection would soon be at an end, and would not require the enactment of a kind of sumptuary law, limiting the number of pupils to be taught by each professor. Dr Clark has not, however, the merit of being the first to make this objection to the clinical school of Edinburgh, for it did not escape the partial eulogist of that of Dublin, although he had the candour to ascribe it to its true cause, and one of which the professors of Edinburgh have no reason to be ashamed.*

Dr Clark's third objection is, that "the professor has too many patients to examine and prescribe for, during the time allotted to the clinical visit, which ought to have for its object the instruction of the pupil as well as the treatment of the sick." We admit the principle, but deny the application, and we assert that the patients under the charge of each professor are *not* too many. Upon this subject we apprehend that the opinion of the professors who have had long experience in teaching clinical medicine is of greater weight than that of speculative re-

* Comparative view of the Schools of Physic in Dublin and Edinburgh, 8vo, Dublin, 1818. "The celebrity which the school of Edinburgh has obtained is in many respects a source of injury; but in no instance is the effect so evidently produced as in the clinical department. The number of students who attend the Infirmary is so great that they interfere materially with each other."

formers, who have never tried to teach, and know not its difficulties or facilities. The smallness of the clinical wards, and the limited number of patients, were always found to be great drawbacks on the value of the clinical instruction at Edinburgh; and it was at the urgent request of the professors that, by the liberality of the Managers of the Royal Infirmary, the present commodious, we may almost say magnificent wards, have been added to the clinical department. Each of the two attending clinical professors has now two wards under his charge, one smaller, for contagious febrile diseases, and another larger, for diseases of all other descriptions. Although great power of admitting patients is thus given to the professor, it is entirely discretionary with him to admit fifteen or twenty, the number to which Dr Clark would limit them, as to extend them to forty, the number of beds the two wards contain. In point of fact the beds are sometimes found to be too few, because, after they are all occupied, a case of very great interest and rare occurrence presents itself and would have to be refused, and thus not brought under the notice of the clinical pupils, if the clinical professor had not the power of ordering an additional bed, until a vacancy can be made by dismissing patients whose cases have terminated. It is no defect in a system that it is possessed of great power, and it is for the master to whom it is given to judge how far he is able to exercise it with advantage. Those who have tried this increased power do not find it above their strength. What Dr Clark would have the world consider as a defect is therefore in reality a great advantage, and his proposed remedy of diminishing the number of patients under the charge of each professor would be a deterioration. Indeed, when we consider that of the acute cases two-thirds at least are generally in the state of convalescence, and of the miscellaneous cases, nearly the same proportion consists of chronic cases, which require only continual superintendence and occasional investigation, the number of those engaging the attention of the professor and the pupils will not be found to exceed, and in reality does not exceed, the limits assigned by Dr Clark.

Another evident and very great advantage derived from the increased number of beds is the more frequent opportunities of investigating the pathology of those cases which terminate fatally, and of comparing the appearances detected in the body with the symptoms preceding death, and with the opinions formed as to its nature. At present the students have an opportunity of witnessing the examination of the casualties which eighty beds afford, and of having the appearances explained.

The fourth objection of Dr Clark is, that "the clinical lectures are too few;" and the remedy naturally proposed is to in-

crease the number of lectures, which Dr C. thinks should, under no circumstances, be fewer than three in each week. This objection certainly does not now apply to the Edinburgh clinical school, and was rather strange even when only two clinical lectures were delivered each week, because more than two have seldom been given in any other place, nor with greater, or even so much regularity. This is candidly admitted by Dr Clark. "These lectures form an excellent part of the clinical system of Edinburgh, and are too much neglected in the foreign universities in general." p. 11. In Edinburgh at present four clinical lectures are delivered each week, during the winter session, often six, according to the state of the wards, at an average five; which not only far exceeds any thing of the kind in any German university, but is perhaps more than the students are able properly to attend to; and we should not be at all surprised to find it objected by another reformer, that the clinical lectures were too many, and ought to be reduced. Surely experienced teachers are the best judges of what they can teach, and what, under the specific circumstances, should be taught, or what may be learned.

Dr Clark's fifth objection is, that "the pupils have little or no opportunity of acquiring any practical experience under the direction of the professor." The remedy proposed for this is to institute a practical clinic upon the principles of the policlinics of Germany. To this we have only to reply, that, as an optional study, this already exists here in a greater extent than in any place in Germany, and that, as a compulsory university branch of education, it is utterly impracticable.

The last objection made by Dr Clark is, that "the clinical professor retains the charge of the clinical ward for too short a time, and resumes it at too long intervals;" and the proposed remedy is, by making the office of clinical professors permanent, or fixed at least for a series of years in the same individuals. Dr Thomson's observations on the subject of clinical medicine are chiefly intended to press the appointment of a permanent professor of clinical medicine, having no other charge. We very much prefer Dr Clark's plan; but we are inclined to think the present system preferable to either.

Such questions may be investigated in two ways, by referring to past experience, and by reasons of expediency. The general practice of the continental universities which have obtained the greatest celebrity, and where the professors are restricted to certain subjects which they are appointed to teach, is, that the clinical lectures are given by the professors of the practice of physic; as at Padua by Brera, Pisa by Morelli, Bologna by Tomassini; at Pavia formerly by John Peter Frank, and after-

wards by his son Joseph; at Vienna, formerly John Peter Frank, and afterwards Hildenbrand.

In the north of Germany, where, as we have already stated, there is no compulsory study, every professor may teach clinical medicine that possesses the essential qualification of being physician to an hospital; and those who have no hospital, but are desirous of giving clinical instruction, set up their own policlinic, as our lecturers do their several dispensaries. At Gottingen, Himly, as Richter formerly did, unites clinical surgery and medicine in the university hospital, while, of his competitors, Conradi has his own clinical institute, and Langenbeck his surgical hospital. Himly teaches also physiology, human and comparative; general nosology, therapeutics, and materia medica; the nosology and therapeutics of the digestive and respiratory organs, skin, urinary, and sexual organs, and the operations for the cure of diseases of the eyes and ears;—while Conradi teaches also general pathology and general therapeutics, with particular reference to practical materia medica; and special pathology and therapeutics. At Leipsic Clarus teaches special therapeutics and clinical medicine, while Cerutti, who teaches also pathological osteology, has his policlinic; at Berlin, Berends has the clinical medicine in the hospital, and he teaches also the principles of pathology and materia medica, and explains Celsus. Hufeland has the royal policlinic, and teaches practical medicine. It is needless to multiply examples, more especially as the subjects taught by the German professors are varied every year according to their own discretion and pleasure. The most celebrated teachers of clinical medicine in our time at Paris have been Pinel and Laennec; and Dr Thomson could scarcely fail to know that both held professorships, and gave regular lectures, distinct from their clinical duties; Pinel in the *École de Médecine*, and Laennec in the *Collège de France*. In short, there is hardly an example of the office of teacher of clinical medicine being separate from other charges, and these of the most multifarious kind.

Contrary, however, to the practice of the best universities on the continent, Dr Thomson is for disjoining the teacher of clinical medicine altogether from the existing chairs. We feel assured that so harsh a measure will not be taken, unless upon the strongest evidence of its expediency. This clinical school was established by the professors of the Medical Faculty, with the concurrence of the Managers of the Royal Infirmary, and under their management it has maintained its reputation, we will venture to say undiminished, for nearly a century. During the last session the number of clinical students attending was not only greater than was ever before known, but bore a greater

proportion to the whole number of medical students. Whether any legal right has been thus acquired, it is needless to inquire, as we are willing to meet the question on the grounds of expediency. We are willing to concede, that a professor, who devoted his whole time to the teaching of clinical medicine, should teach it well; but from having no other academic duty, he would be more apt to employ the time not necessarily occupied in his clinical occupations, in pursuing private practice, or following other pursuits not academical. Besides, were the whole clinical teaching confided to one individual, the great improvement lately introduced, of having two professors dividing the attending students, would be lost. It is also an important consideration, that the discharge of the clinical duties requires a constant state of vigorous health, which cannot be expected in any individual; nor could his place be well supplied upon an emergency, by a colleague altogether unaccustomed to the details. The natural decay of advancing years must at last unfit every man for the discharge of so laborious an office, and often long before his decease. An assistant might then, no doubt, be appointed, but his proper remuneration would be a matter of difficulty, and there would a constant risk of this most important chair being inefficiently filled. Another danger would arise from the total want of competition; for whereas all the other subjects are ably taught by non-academical lecturers, whose tickets are received by the college of surgeons, and other public boards, the clinical teacher alone, from want of other hospitals, has no such competition to stimulate him to exertion, but under the present system its good effects are secured by the friendly emulation existing among the professors themselves. Dr Thomson seems to have been aware of this objection, and has suggested that the ordinary physicians of the hospital should be encouraged to give clinical lectures; in which suggestion we agree with him, even were the academical clinical teaching to remain on its present footing, as there would then be no hardship in excluding from the academical clinical wards all students except those attending the lectures of the professors. Were there but one clinical professor, moreover, the students would have little opportunity of seeing any variety of practice; and it is undeniable that almost every practitioner falls sooner or later into a routine, from which, especially in advanced life, he seldom deviates.

Even upon the clinical instruction, therefore, we think that the present system of voluntary rotation confers advantages, by securing always an adequate number of efficient teachers. Indeed this has appeared to some so great an advantage, that when at one time, owing to the causes we have pointed out, a smaller number of the faculty undertook the clinical duty, the

Dublin and Edinburgh schools were thus contrasted, to the advantage of the former. "In Dublin, the six professors of the school of physic fill this office in rotation, holding it each for three months. In Edinburgh, it is exclusively confined to two of them." *Comparative View*, p. 36.

But upon the professors teaching the various branches of medicine, even those less directly connected with its practice, their taking their turns of the clinical lectures has effects of the most advantageous description. They are thereby led to preserve more directly the connection of their respective chairs with the profession, of which otherwise they might lose sight too much. It is urged that a professor of chemistry or botany might be appointed, who was not a physician, and incapable of giving clinical instruction. This is no doubt true; but in this university it has never happened, and it rests with the patrons to prevent the occurrence; because, if they judge it right to give the appointment of Professor of Chemistry or of Botany to a person whom they do not think qualified for the office of clinical teacher, they have only to withhold from him the title of Professor of Medicine. Even if no precaution were taken, there is little danger of any one inadequate or not a zealous cultivator of practical medicine engaging in this duty. Many of the Edinburgh Professors, although enjoying the privilege, have never availed themselves of it. On the other hand, being profoundly learned in the accessory branches does not disqualify a physician from being a good clinical teacher. When Cullen began he was Professor of Chemistry; Monro was Professor of Anatomy; Whytt of the Institutes; D. Rutherford of Botany. On the continent also we find the like connection; Boerhaave, when teaching clinical medicine at Leyden, then the first university in Europe, taught likewise both botany and chemistry. To the other chairs, the practice of physic, materia medica, and the institutes especially as connected with pathology, hospital practice is almost indispensable. To the cultivation of these branches, private practice, even the most extensive, contributes little. The professors cannot bring the knowledge derived from books, and the new medicines and practices recommended to the sure test of experience, nor decide by personal experience and the frequent examination of the bodies of the deceased, anywhere else nearly so well as in hospitals, and in the presence of their pupils.

There cannot be a doubt that giving clinical lectures, instead of distracting the attention of the medical professor from his peculiar province, increases greatly his fitness for teaching it. This is the opinion of the most philosophical writers upon the subject. "The practical teacher, when at the bedside of a patient, is in his true and almost only infallible place of study. Here he draws from nature herself, and if he renounces this

sketch to study copies, and to seek in books the fancies of a heated imagination, it is evident how little he and his pupils will benefit from it."—Frank. "La pratique médicinale est fondée sur l'expérience; ceux qui prétendent à l'enseignement public, doivent donc avoir vieilli dans les hopitaux, dans l'exercice de la médecine clinique."—Gilibert; l'Anarchie Médicinale. Accordingly, Dr Clark admits, that "when the Medical Professors of the university have the time and inclination to take part in the charge of the clinical course, as an equally regular and fixed duty with that of their other professorships, they should be preferred." p. 22. We are not inclined to quarrel with any man because he differs from us in opinion. We grant to others the same liberty of thought that we claim to ourselves; but we confess, that we feel hurt in being told, that at Edinburgh "the clinical duty appears rather a secondary consideration." On this point we conceive ourselves to be better informed than strangers can be; and we appeal confidently to the hundreds of clinical pupils who have attended the present professors, whether the regular exercise of their clinical functions has not always been with them a paramount duty. Indeed this is admitted by Dr Clark, when he says that he "can most sincerely affirm his belief, that in no university in any country are men to be found more highly qualified for teaching clinical medicine, or more conscientiously disposed to perform the duties of clinical professors." If so, we might presume that they would be the persons best qualified to make improvements on the system.

Dr Clark considers it as a bad regulation that the professors of the medical faculty should be allowed to "take upon them the duties of clinical professor, or not, just as suits them." We hold the opposite opinion, and contend, that it is this liberty which has always secured for the Edinburgh school, willing, zealous, and competent teachers of clinical medicine.

It is also objected to the present system, that the rotation of clinical teachers is too quick; and the arguments which were used by Dr Gregory against the quick rotation of surgeons of the Royal Infirmary, formerly in use, are brought up in array against the clinical professors, and are said by Dr Thomson, to be applicable, *mutatis mutandis*, to the present system of medical clinical instruction. This was not Dr Gregory's opinion, for in the very same memorial he was at pains to point out the difference, in answer to Mr John Bell, who attempted the same line of argument as Dr Thomson. This passage has not been quoted by Dr Thomson; we shall supply the defect.

"It may be observed, that, in the long passage last quoted from the answer to me, Mr John Bell labours hard to point out some resem-

blance between the system of indiscriminate attendance of the surgeons by rotation, and the attendance of several of the professors of physic in rotation, with a view to give clinical lectures, and of course states the matter in such a way as to give the preference strongly to the mode of attendance of the surgeons. Whatever resemblance there may be between the two cases, there can be no difficulty in pointing out an infinite difference between them. In the first place, the number of medical professors in this university is but small. There are but six such professorships, and sometimes two of these, that of Botany and Materia Medica, have been held by the same person. The professors of physic, whatever their merit or demerit may be, have at least been *selected*, by the patrons of the university, from among a great number of physicians, who would have been ambitious of that appointment. The patrons of the university have not even confined themselves to a choice among the great number of physicians settled in Edinburgh. They have occasionally given the appointment of medical professor in this university to physicians who had acquired reputation as professors in other universities.

"Such was the case with Dr Cullen, Dr Black, and the present Dr Hope, who were well known and highly esteemed in the university of Glasgow, and with my father, who had exerted himself to the utmost in teaching several branches of medicine in King's College, Aberdeen, before they were appointed professors in the university of Edinburgh. From such examples, and from the still more striking general fact of the high reputation which the medical school of Edinburgh acquired in less than half a century, it may fairly be presumed, that the patrons of it were equally honest and public-spirited, in the choice which they made of the other medical professors, and that upon the whole they have been tolerably successful in that matter; though no doubt Mr John Bell and his clients will think it right to make a great deduction from their credit in that respect, on account of their having twice bestowed the appointment of professor on so ignorant and worthless a person as their very humble servant.

"Nobody can suppose that the patrons of the university shall always be directed by a kind of inspiration, to make choice of men of superior genius, as professors of physic, or of any other science in this university. Nor can any body suppose that their appointment to a professorship is to confer talents on a man which nature had denied him. But though I am, by their appointment, one of the medical professors myself, I must do them the justice to say, that they have had the sagacity to contrive, and the honesty to employ very faithfully, a simple but effectual expedient to prevent their professors from being either ignorant or lazy. The expedient is indeed so admirable and infallible, and withal so cheap, that it ought to be generally known. It consists in giving us very small or no salaries, so that our emoluments, which for a long time past have been very considerable, arise almost entirely from the number of our pupils, and this again from our own diligence in learning and teaching the several branches of science which we profess. I trust

Mr John Bell and his clients will know where I got the two following lines, and, whatever they may think of the poetry, will at least acknowledge the truth of them.

*Qui docet indoctos, licet indoctissimus esset,
Ipse brevi reliquis doctior esse queat.*

"A professor of physic in this university who does not seriously study and understand his own profession, must, in the first place, become an object of contempt to his colleagues and to the students; and, in the next place, he must starve, for the students will not attend his lectures, and the reason of this must be immediately and generally known.

"On these principles, I should conceive, *a priori*, that there must be an infinite difference between the attendance of the whole six medical professors by rotation in the clinical wards, if this should ever happen, and the attendance of twenty, thirty, or forty surgeons by rotation in the surgery wards, just to acquire a little experience and dexterity in their profession. But the well known fact with respect to the attendance of the medical professors in the clinical wards, puts this point in a much clearer and stronger light." *

Here follows the passage quoted by Dr Thomson, p. 42, Additional Hints.

In another passage Dr Gregory gives some additional explanation concerning the effect of the rotation of the clinical physicians, quite sufficient, as we conceive, to set aside the objection of Mr John Bell and Dr Thomson to the present system.

"*The first physician comes out in November, fills his ward, assort his diseases, writes notes and regular reports of his patients complaints, completes his experiments, lectures on their cases, and then empties these wards, by delivering his patients over to the ordinary physicians, or by actually dismissing them from the house.* Every clinical professor I believe makes it a rule to take in very few new patients for some time before he ceases to attend in the clinical wards, so that when his time is expired he may not leave any great number of patients whom it might be inconvenient to his successor to keep in those wards, or to the ordinary physicians to receive at once into theirs. This, which cannot be done in the common wards, either medical or chirurgical, *Patet omnibus* being the fundamental rule and well known motto of this infirmary, is easily accomplished in the clinical wards, by taking in, during the few last weeks of the professor's attendance, only acute cases, or some of the most urgent of the chronic. But when it happens that there are in the clinical wards patients labouring under obstinate chronic diseases, who are so uncivil that they will neither die nor recover, such patients as Dr Cullen used emphatically to call the *stick-fasts* of a clinical ward, in plain common sense, the best thing that can be done for them, is to commit them to the care of another physician. This is giving them every chance for recovery, and it is even positively en-

* Additional Memorial to the Managers of the Royal Infirmary. By James Gregory, M.D. &c. 4to, Edinburgh, 1803.

joined by the rules of the infirmary, with respect to the patients of the ordinary physicians. And as to the convalescents, who must occasionally be left by a clinical professor in the hospital, it is hardly conceivable that they should suffer any injury by going under the care of another physician."

If the clinical teachers should become satisfied that their rotation is too quick either for the good of the patients, or for the advantage of the pupils, the remedy is easy, and in their power. But the inconveniences of quarterly rotation have been greatly exaggerated. In some respects it is preferable to the same teacher continuing longer in attendance. Medicine is now cultivated chiefly as a science founded upon observation and experience, and there is little danger of students being distracted by opposite dogmatic principles. The treatment of some diseases is well understood, and the whole difference among practitioners is in their degree of activity. In witnessing such diseases under the care of different teachers, the pupil has an opportunity of judging for himself how far nature, with a little assistance, is able to effect a cure, or how far interference with her efforts may be carried, with advantage or with safety. In another set of diseases no plan of treatment is so decidedly beneficial that it is universally preferred; and very often the palliation of symptoms is all that we expect, or can obtain. In these diseases, after one kind of treatment, or even many varieties of treatment have failed, another may succeed. Practitioners, even the most skilful, are very apt to fall into a routine practice which is too little varied in different circumstances, and the pupil, after becoming acquainted with it, relaxes in his attention. On the patient suffering under obscure and chronic disease, the change of practitioner has often marked good effects, by the stimulus of excited hope, and by the change of remedies which, from continuance, had lost their influence. The opportunity of observing a variety of practice in such cases does not confound the judgment, or shake the principles of an intelligent pupil, but, on the contrary, has upon his mind salutary effects far more valuable than the system of inculcating dogmatic principles and routine practice. It is to this circumstance that we may chiefly ascribe that readiness to receive new information, that liberality of understanding, that independence of sentiment, that habit of thinking and reasoning, *nullius in verba magistri*, for which the Edinburgh pupils have been long distinguished.

Additional optional clinical study cannot be too strongly recommended, and the high estimation in which it is held by the medical faculty is evident by the privilege conferred on hospital attendance, in shortening the period of academical instruction.

But we doubt whether the kind of clinical study, recommend-

ed by Dr Clark to be increased as a compulsory measure, is altogether the best, viz. the addition of a whole session of clinical surgery, and another of attendance at the practical clinic.

While far from denying or detracting from the value of clinical surgery, we cannot avoid noticing the unwarranted partiality shown in the pamphlets before us to this branch over clinical medicine, as they are at present taught at this school ; for all the objections brought forward to the latter apply more strongly to the former, besides others peculiar to itself. These objections arise from circumstances which it is not in the power of the highly esteemed professor, or of his able coadjutors, to remove. The professor, from having no patients under his own immediate charge, and being thus obliged to lecture upon the cases of patients treated by other surgeons, cannot possibly render his lectures truly clinical. He cannot lay down a plan of treatment which it is his intention to follow ; he cannot fully explain the reasons which may have guided the practice of the attending surgeon ; and his delicacy will not permit him to comment with freedom upon errors in diagnosis, prognosis, and treatment, as discovered by examination after death. Yet the exposition of the fallibility of our skill is one of the most valuable parts of clinical instruction. Again, if the course of clinical medicine be too short, that of the surgery is still shorter ; the one being continued uninterruptedly during the whole winter session, the other leaving two of the six months unemployed. If one or even two medical professors are too few to teach the number of medical students of Edinburgh, how is one surgical professor to teach all these students, with the addition of the numerous surgical students who do not attend clinical medicine ? We cannot help adverting to the predilection for surgery evinced in the repeated pressing of its extended study in various forms upon students who do not mean to practise it, while there is no recommendation by these reformers, so careful of the public weal, of attendance upon clinical medicine for those students who mean to content themselves with the surgical diploma, although to them it is as useful as to the medical graduate ; for it is medicine, and not surgery, that is the business of the great mass of surgeons and general practitioners.

On this subject we suggest the following method of extending clinical instruction as practicable in the first instance, leaving it to be further extended, if it be found that it does not press too hard upon the students.—Let three courses, of three months each, be given of clinical medicine and of clinical surgery, and let it be required of the medical graduate to take two courses of the medicine, and one of the surgery ; and of the surgeon to take two of the surgery, and one of the medicine. This should, however, be accompanied with a regulation, by which those

gentlemen, numerous with us, who have already taken the surgical diploma in other schools, as London or Dublin, and do not come to Edinburgh for surgical instruction, should be exempted from the attendance on our clinical surgery.

The policlinics of Germany seem to have fascinated Dr Clark, and yet it appears to us that he has not understood them in all their bearings. Even as to the name, expressive of its meaning, he is wrong. He tells us that the *polyclinic* is "so called from the much greater number of patients seen at this than at the hospital clinic." p. 7. But our German friend writes us, that the term is *policlinical*, from *πολις*, a city, not *polyclinical*, from *πολυς*, much. Hufeland's valuable reports, which we always peruse with much satisfaction, are of the Royal Policlinical Institute of Berlin. In short, in plain English the policlinics are city dispensaries, and the only novelty consists in the name.

Dr Clark's suggestion proceeded on the idea, that attendance at a policlinical institution is a branch of University education, and necessary on the continent for obtaining a degree; but this is not the case. They are entirely private concerns, established by individual teachers, and attendance upon them is no more compulsory than attendance on our dispensaries. Their insertion in the University programme of the classes taught, is according to the practice of inserting every thing which any professor teaches or offers to teach, whether connected with any profession or not. Thus Wrisberg, the anatomist at Gottingen, advertised in the University schedule, and delivered lectures on the objects to be seen in a journey from Trieste to Vienna, and illustrated this subject by an extensive collection of engravings. Our German correspondent says that the policlinics "are usually arranged by an agreement between the Professors and the Town Council, as they take a domiciliary care of the sick poor; less frequently the government takes an interest in these matters; but the hospital clinics being confided to the senior professors, the younger professors are usually confined to embrace only the policlinicum." These are the words of our valuable and learned correspondent, which we have left unchanged, that we may not misrepresent his meaning, which appears to us to be that the policlinic is undertaken by, or entrusted to the younger professors only.

From the following account given of the Royal policlinic of Berlin in the University Calendar, it is evident that *attendance upon it is optional*,—and that the liberty of practising under the superintendence of the teacher, recommended as essential to all our graduates, is limited to twelve only of the numerous students in the metropolitan University of the Prussian dominions.

"The apartments of the Policlinical Medico-Chirurgical Institution of the University are in the University Buildings. They consist of a hall, and a clinical laboratory for the instruc-

tion of the students in the knowledge and dispensing of medicines. In this hall, the director, his two assistants, and the students, meet daily, partly for the purpose of admitting and giving advice to new patients, and partly to receive reports upon the cases of those under treatment. The poor patients are furnished with medicines *gratis*, at the expence of the king; those confined to bed are visited in their houses, and, when necessary, furnished with food, fire, wine, &c. at the expence of the institution. The average number of patients treated annually is from 1000 to 1200. The students who attend the institution are divided into two classes, hearers and practitioners; and of the latter the number is *limited to twelve*, and these must have previously given sufficient proofs of their skill.* In fact, they have almost all previously taken their degrees; as in Edinburgh many of our graduates remain for a term to become practising pupils at the dispensaries.

As an optional method of acquiring professional experience, the Dispensaries in Edinburgh will bear a strict comparison with the most renowned Policlinic of the north of Germany, and, taken together, they give practical instructions to many more pupils than the Policlinic of Berlin.

But if we understand Dr Clark's proposition, it is, that a year's attendance upon an University Policlinic be compulsory on every candidate for a degree of medicine; in which case his plan is visionary and impracticable, not only in Edinburgh but in Berlin. The medical officers of our dispensaries find two practising pupils each as many as they can look after consistently with their duty of making themselves acquainted with each case; and if there be a hundred candidates in a year, there are not teachers enough, even if the whole medical officers of all the medical institutions of Edinburgh were employed. Much less can such a mode of teaching be limited to professors in the university. The Senatus Academicus have shown how much they value this mode of clinical instruction, by allowing one of the four years of professional study, now made imperative, to be employed in attendance upon the study of medical or surgical practice in any hospital of sufficient size; thus conferring on the medical officers of every large town in the kingdom the status of a policlinical teacher. It is right to mention, that since the additional wards have been put under the charge of the clinical professors, arrangements have been made, by which about twenty-five students may be annually employed as clerks in these wards alone. If we add to them the gentlemen employed as clerks to the ordinary physicians and surgeons of the hospital, it will appear that nearly one-third of the graduates of

* Jahrbuch der Königlich Preussischen Universitäten, Erster Bändchen, 8vo, Berlin, 1824.

each year may in future be initiated into the duties and responsibilities of their profession, in the Royal Infirmary only, besides what is done in the dispensaries.

It is quite a mistake, however, to suppose, and in fact is not expected by the public, that graduates are to be sent from the university experienced practitioners. All that can be done in schools is to prepare them for benefiting by experience as opportunities offer. These opportunities of acquiring experience do offer to all, both during and after the period of their studies, first, as spectators of the practice of others, afterwards as assistants to senior practitioners, and lastly, as practitioners themselves. A great school may take cognizance of all such means of practical improvement, wherever employed, but cannot itself afford instruction in all these ways to all who may find their advantage in resorting to it. Nor can such instruction be limited in time, more than in place. As long as we live we are acquiring experience. The object of clinical instruction in a great school is to give the greatest experience in the shortest time; and this end, in a numerous class of students, is more effectually attained by the whole observing the practice, and hearing the remarks of the teacher, than by each attempting to practise before the others. Those who have ever tried this mode of teaching know what a waste there is of time, and how irksome it is to the whole class.

To the plans of improving the teaching of clinical medicine, proposed by those who never tried it, we may apply in general the language used by Frank in regard to the project of Olivari, submitted for his opinion by the senate of Genoa. “Je regard donc le dit projet plutôt comme la *métaphysique* de la *clinique*, que comme un plan de règlement pour l’instruction des jeunes medecins;” and he adds, “un plan d’école clinique à mon avis, doit laisser plein liberté au professeur, qui doit entrer en cette carrière, la manière dans l’examen, dans la disposition des maladies, et ne doit fixer que les principes d’administration et une méthode simple et aisée, qui puisse conduire à la meilleure education du jeune praticien.”*

Dr Clark has alluded to the clinical attendance given in some foreign countries after graduation, before the liberty of practising in these countries is conceded. This, therefore, has no reference to the curricula for graduation; and its enforcement belongs to the Legislature, and to the Royal Colleges of Physicians, not to the universities. In the northern universities, such as that of Berlin, it is not understood to be a method of giving additional clinical instruction, but is a mode of state-examination, whether the *Doctor* already promoted has sufficient

* Plan d'Ecole Clinique.

practical skill, as well as theoretical science, to obtain the right of *free practice* in that particular state.

Before concluding our observations on this subject, we must take notice of Dr Clark's assertion, "that the value of clinical instruction has been very differently estimated in this country and on the continent;" and that it has never received in this country the consideration which its importance among the branches of medical education demands. We are at a loss how to reply to this assertion. It is only within these few years that clinical medicine has received on the continent that consideration among the branches of medical study which it has held in the school of Edinburgh almost from its commencement. Dr Clark himself admits that the university "has been indebted to her clinical school for much of her reputation." At this time it is the best clinical school in the British empire. Neither in the English Universities, nor in London, are any regular clinical lectures delivered. That of Dublin is yet "in its infancy."*

The regular teaching of clinical medicine is of very recent date in France. Prunelle in 1816 reproaches his native country with the tardy introduction of clinical instruction. "C'était donc en vain que la renommée publiait depuis long temps les succès des cliniques de Leyde, de Vienne, d'Edimbourg, et de Pavie; aucun institution de ce genre ne s'élevait parmi nous, et la France a vu naître l'enseignement clinique presque au milieu de la plus effroyable tourmente,"† p. 59.

Even in Germany it was very imperfectly taught as a branch of university education, except in Vienna. Indeed, the smallness of the cities in which the most celebrated schools were seated, as Gottingen, Halle, Jena, precluded the necessary supply of patients, which rendered the introduction of the policlinical system almost a matter of necessity. Frank in 1790 quotes‡ Edinburgh and Vienna as "*les meilleures écoles cliniques*;" and in 1817¶ he still thinks it necessary to combat such arguments as the following against the propriety of establishing them.

"The opportunity of acquiring practical skill is foreign

* "The exclusion of the certificates of other universities has, with respect to clinical tickets at least, been, by a distinguished physician defended, on the ground that such monopoly is necessary for the protection of the Dublin clinical chair as an institution yet in its infancy." Observations relative to some Defects of the Medical School of Dublin, in a letter to the Board of Trinity College. By John Clendinning. A. M. M. D., 8vo, Dublin, 1827. This may indeed justify Dr Clark's assertion as to the little consideration given in this country generally to clinical medicine, but not in reference to this university, to which his remarks must be understood to apply.

† Des Etudes de Medicin, 4to, Paris, 1816.

‡ Plan d'Ecole clinique; ou methode d'enseigner la pratique de la medecine dans un hopital academique. Par Jean Pierre Frank, 8vo, Vienna, 1790.

¶ System, Vol. vi. p. 335 and 441.

to the purpose of a university, and it may become injurious, by misleading the students to neglect their proper university pursuits, the acquisition of profound scientific knowledge.”*

“In fact, it is not easy to define accurately the limits between theoretical and practical academical instruction; and much depends upon beginning the practical exercises neither too soon nor too late, and upon attending to them neither too much nor too little. Upon the whole, it seems less injurious to begin practice too late than too soon, and to blend too little than too much of it with the other studies. The object of practical instruction at universities is rather to excite the attention of the pupils to the practice of their teachers, by ocular demonstration and clear explanations, than to impart to them a more than common degree of skill.” p. 442.†

The policlinic is an institution of very recent formation. The first seems to have been established by Hufeland at Jena, at least as early as 1794.‡ The Berlin policlinic was instituted also by Hufeland in 1810. We leave our readers to judge how far it can be justly said of Edinburgh, that she will “be ultimately compelled to follow in the march of improvement which she should have led.”

It has become customary to accompany and enforce the various recommendations of reforms in our medical school, with which we have lately been favoured, with terrible denunciations of the fate that awaits us, if we obstinately adhere to our old institutions, and neglect the warning voice. We are to be doomed to follow where we ought to have led; our halls are to be deserted, and our names forgotten, &c. &c. If this be meant only as a “hint of what we can do in the sublime,” we should be sorry to deprive our friends of so copious and easy a theme of declamation; but if any of our readers suppose that the conduct of this university, in regard to medical education and the medical degree, really calls for such animadversions, we beg to remind them, *first*, that it was *in this university* that the additional year of study, and the increased hospital attendance, (certainly the truly important parts of the changes lately made in different schools of medical education,) were first made imperative on medical graduates; and *secondly*, that it was the *avowed willingness* of the medical professors here, to make certain reforms, and to consider of others, which brought

* Wedekind, Ideen zur Polizey der Heilkunde; in Kepp's Jahrbuch der Staats-arzneykunde, vii Jahrgang.

† Weber, Versuch ueber die Errichtung und Einrichtung der Universitäten, 8vo, Berlin, 1805.

‡ Nachrichten von der medicinisch-chirurgischen Krankenanstalt zu Jena in Hufeland's Journal der practischen Arzneykunde und Wundarzneykunst, 8vo, 3ter Bd. Jena, 1797.

the subject before the medical public, and gave Dr Thomson and Dr Clark themselves the opportunity of addressing to the patrons and professors of the university, the very suggestions which we have now been considering.

In fact, the alterations that have been made here, in the last few years, both in the mode of teaching, and in the amount of study demanded, have been extensive and important. The medical professors are far from claiming to themselves the credit of all these changes. Much is due to the liberality of Government and of the Commissioners for College Buildings, and we willingly add, to the Patrons of the University; and much to the zeal and ability of other lecturers;—but all the changes to which we allude are clear gain to the medical students of Edinburgh, and contribute to qualify them for the medical degree.

The extensive anatomical museums, belonging to the University and to the College of Surgeons, are now arranged and exhibited regularly; the students of chemistry have the advantage of excellent opportunities, both within and without the university, of acquiring minute and practical acquaintance with that science; the lectures on botany are delivered in a splendid and daily improving garden, and are much more numerous and minute than formerly; the lectures on *materia medica* are illustrated by the performance of many of the pharmaceutical processes, and by the exhibition of a large and increasing museum; we need not say that the kindred sciences of natural history and comparative anatomy are taught both by lectures and by the exhibition of museums, in and out of the university, in a style of excellence formerly unknown in this country; the class of the practice of physic, formerly overburdened, has been relieved, and at the same time, instructions which may always be expected to be practically valuable, in regard to the diseases peculiar to women and children, have been secured by the extension of the lectures of the professor of midwifery; on all practical subjects there are able and zealous private lecturers; and the opportunities of clinical instruction in the clinical wards alone have been more than doubled, besides the establishment of additional dispensaries, which act as practical schools, and are under good regulation.

The practice of examination of their respective students by the professors has been introduced into several classes, and may probably be carried farther, though the professors do not wish to come under an obligation as to the extent to which they may find themselves able to undertake this laborious duty. The examinations for the degree are stricter and more minute than at any former period, extending, in the case of almost every individual, to two hours, and in many, by adjournment, to four

hours ;* and an arrangement is now under consideration, and will, in all probability, be carried into effect next season, by which we have no doubt these will be materially improved, viz. the division of each private examination into two parts, one before the professors of the more elementary, and the other before those of the more practical branches, with an interval, never less than some weeks, between the two. By these means the distraction and embarrassment naturally resulting from the great variety of subjects on which the candidate has now to prepare himself for a single examination, will be diminished, and the examinations may in consequence be rendered still stricter, without being in reality so formidable, at least to a student of regularly industrious habits.

To the alterations lately made in the course of study requisite for the degree, (and which are detailed and explained in our twenty-third volume, p. 421,) we shall only advert, on the present occasion, to observe, that next year will be the first in which the candidates will be required to conform to these new regulations, and that, in our humble judgment, it will be prudent to have some little practical experience of the working of the new system, before proceeding to farther changes in this department. It will be seen from what has been said above, that others are already in the contemplation of the professors. Of course we know nothing of the views of the Royal Commissioners ; but we have no doubt that we shall carry our readers along with us when we say, that, in order to prevent well-grounded complaints and endless confusion, any future statutes affecting the course of study ought to be accompanied by the same explanatory clause which unluckily gave so much offence to our worthy Patrons in the last, *that they shall only be imperative on those who shall commence their medical studies subsequently to their publication.*

It has been said that the medical professors of Edinburgh are inferior to their predecessors. They can take no credit to themselves for modesty in admitting this fact, being persuaded that, if it be a disgrace to be inferior to such men, it is a disgrace which they share with all their professional brethren of this age and country. But when it is farther said or insinuated, that they are “grown impotent of toil,” and careless of the duties they owe to the university and to the public ; they will fearlessly appeal to the testimony of thousands in support of the assertion, that they devote their faculties, such as they

* In proof of the importance of strict examination, particularly on anatomy, we may mention, that although the opportunities of dissection in universities are not such as to enable the Senatus to make a course of practical anatomy absolutely imperative on graduates, there has been hardly an instance, within the last three years, of a candidate coming forward who had not gone through at least one such course somewhere ; and a great majority have had more.

are, to the duties of their station, as willingly and faithfully as any of their predecessors. If they hesitate to adopt all the suggestions of speculative reformers for the improvement of medical education, it is not because they are unmindful of that part of their duty, but because they are convinced that in this country the qualifications required of those who are to enter a practical profession, and to have no monopoly of its exercise, must be limited by the practical demands of the public; that if the requisitions of universities for the medical degree are pushed farther than these demands justify, the ultimate result must be a diminution, not an increase, of the average attainments of practitioners;—and that the way to judge of the actual demands of the public is, not by the speculative opinions or wishes of individuals, however learned and respectable, but by the evidence of facts.

To conclude: Reforms in education, like improvements on government or legislation, ought neither to be rashly nor extensively attempted; but, rising out of the events and circumstances of the persons educated, they should be deliberately introduced according as the experienced teacher, who observes the enlarging views of successive generations of students, discovers that the progress of science and the wants of society demand more extensive, more minute, and more various information. Professor Jardine of Glasgow, whose opinion on these subjects has deservedly obtained so much consideration, has expressed himself thus strongly in regard to the source from which beneficial reforms in education are to be expected.

“It is, however, worthy of remark, that whatever changes for the better shall be made on our system of education, they must begin with the teachers themselves. The art of teaching, like all other arts, is chiefly founded on experience. Improvements, therefore, are not to be expected from legislators and politicians, who have many other objects to engage their attention, nor even from men of science, unless they have had experience in the business of education.”* In quoting this passage we do not mean to express a doubt as to the propriety of our academical regulations being subjected, both to the public scrutiny and to the judgment of such individuals as compose the Royal Commission. We expect nothing but advantage from both; and we are confident that those, who are the best qualified to form a judgment in regard to them, will be the least disposed to do so, without first carefully considering such explanations as we have given in this article, many of which can be furnished only by persons themselves engaged in the duty of teaching.

A. D. Jr.

* *Outlines of Philosophical Education.* By George Jardine, Professor of Logic in the University of Glasgow. 8vo, Glasgow, 1825.

[Addition to the Original Communications.]

ART. XIV.—*Case in which a Lost Nose was Restored.* By ROBERT LISTON, Esq. Fellow of the Royal Colleges of Surgeons in London and Edinburgh, Lecturer on Surgery, &c. In a Letter to the Editor. *With a Plate.*

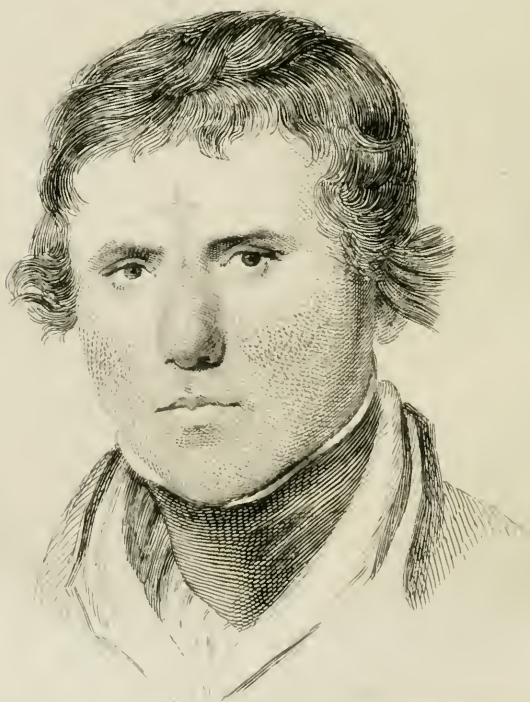
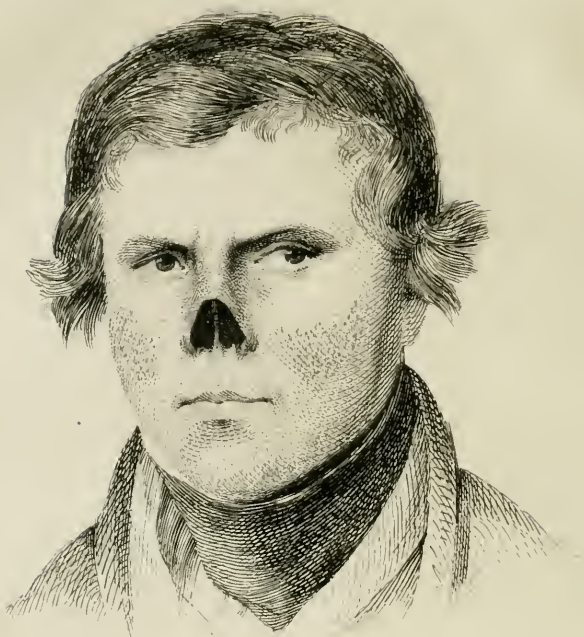
DEAR SIR,

I HEREWITH send you two drawings of the young man THORNE, on whom I lately performed the *Indian operation* for the restoration of a lost nose. The first sketch represents his features as before the operation; the other his appearance six weeks after it.

Mr Carpue of London, in an excellent treatise on the subject, has so very fully entered into the history of the operation as performed by Branca, Taliacotius, and others, from the arm, as also of the one he preferred, and in which he has been followed by Mr C. Hutcheson, that it only remains for me to relate shortly the particulars of the case which came under my notice.

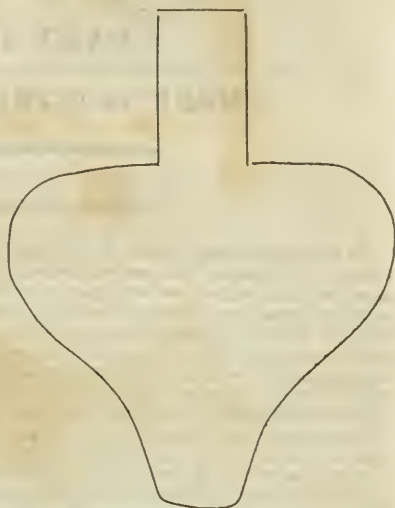
According to his own account, my patient, in helping himself to some apples in an orchard, was struck by the gardener over the nose; ulceration followed, by which the whole cartilaginous part of the organ was destroyed. The ulceration also extended over the cheek superficially, and the cicatrices are still perceptible. When I met *Thorne* he wore a piece of painted paste-board in place of a nose, upon removing which, a large cavity was exposed, giving him, of course, a very disgusting appearance. The sore was completely cicatrized, and had been sound for a considerable time. He expressed his willingness to submit to anything by which his appearance was at all likely to be improved. I accordingly, after procuring a sketch of his features, sent him to my class-room, and performed the operation, with Mr Mackenzie's assistance, and in presence of Dr Monro, several professional gentlemen, and above a hundred of my pupils.

A card of the exact shape and size represented below was adapted to the truncated organ. In another case I should prefer a piece of thick white leather to this, or to the wax employed by the Indian operators. The edges of the cicatrix were then cut away deeply all around, and an indentation was also made on the upper part of the lip for the reception of the columna. The card was then placed on the forehead, with one point resting betwixt the eyebrows, and the other touching the hairy scalp; a knife being carried around it, the card was removed, and the flap, con-



Eng^d by J. Smith

sisting of skin and tendinous expansion, dissected down. The slip was left narrow, but thick, and containing a good deal of muscular substance. The bleeding having stopped, the flap was turned round, and attached by five points of suture, one in the columna, and two on each side; tubes were introduced into the nostrils, and the part lightly covered. Adhesion took place everywhere, excepting at the column; but I still intend attaching this part, and would have done so ere now had the patient not been averse to it, some person having foolishly told him that danger might result. The temperature of the new nose always remained natural, and it retained the disposition to perspire more readily than other parts of the face for some time. The connecting slip was divided and laid down at the end of the fourth week, without any apprehension of the new nose suffering; indeed pressure had been previously applied over the slip so as to diminish its size, and render the part below independent of supply from that quarter. At first, as Mr Carpue has remarked, the flap was loose and flabby, but it soon gained consistence and support by granulation from its inner surface. The surface from which the columna was removed had healed on the removal of the dressings, and the rest of the sore contracted very rapidly; in a short time the probability is that no scar will be perceptible.



This curious operation is occasionally practised in India by the native practitioners. Dr M'Whirter, who paid great attention to their surgery during a long residence in the East, and whilst engaged in journeys into the most remote and unfrequented parts of the country, informs me that these operators are in the habit of pummelling the integuments of the forehead with the heel of their slipper, so as to excite the circulation before performing their incisions. The ingenuity displayed by them in lithotomy on the gripe, and in other operations, is also very extraordinary.

Edinburgh, 9th June 1827.

PART III.

MEDICAL INTELLIGENCE.

MEDICAL CHEMISTRY.

On the Composition of certain Black Vomittings and their analogy with the Black Matter of Melanosis. (*Journal de Chimie Médicale, Septembre 1826.*)—M. Lassaigne has recently analyzed with great care the black matter vomited in a case of scirrhus ulceration of the stomach, and has found that its colour, as was conjectured by M. Breschet, is owing to blood, altered in its properties nearly in the same manner as in melanosis. The patient had long been evidently affected with scirrhus of the stomach, was seized latterly with profuse vomiting of a black fluid, and after death the whole pylorus was found scirrhus and ulcerated, with an aperture communicating with the cavity of the third curvature of the duodenum.

The matter vomited had a chocolate brown colour, and held in suspension darker-coloured flocks, which gradually fell to the bottom, leaving a reddish-brown fluid, resembling a solution of altered blood. The fluid was acid, but gave, by distillation at a low heat, a very alkaline liquid containing much ammonia. The residue was strongly acid, gave out much ammonia with potash; yielded to alcohol a solid acid possessing the properties of the caseic acid procured by Proust from old cheese,—a fluid acid which M. Lassaigne pronounces to have been the lactic, but which, according to the late corrections of Berzelius, the discoverer of that supposed acid, must have been modified acetic acid,—and a little osmazome; and the residue after the action of alcohol consisted of a principle intermediate according to M. Lassaigne between mucus and albumen, but which, from the properties he mentions, appears to have been casein. The dark flocculent matter was evidently altered blood, for weak sulphuric acid acquired a reddish-brown colour when gently heated with it; the solution yielded with ammonia reddish-brown flocks, which, when dried, resembled exactly the dry colouring matter of the blood; the alkalis acted on the original flocculent matter as on blood; and when incinerated, it gave distinct indications of the presence of oxide of iron and phosphate of lime.

The analogy between the sediment of the matter vomited in this case, and the black matter of melanosis, is therefore very great. The presence of the caseic acid has not to our knowledge been remarked in any of the natural solids or fluids, and consequently, as M. Lassaigne suggests, it appears to be a pathological formation.

Adulteration of Sulphate of Kina with fatty matter, and the mode of detecting it. (*Journal de Chimie Médicale, Septembre 1826.*)—The sulphate of kina is now so completely established as an article of the *Materia Medica* of France, Italy, and Britain, that it is not only prepared in these countries in large quantities from bark, but is also imported ready made from the new world. Its value can no longer be called in question. It is of great consequence, therefore, to secure the practitioner and the druggist against its adulteration, which unfortunately is too easy to accomplish, and not always easy to detect. The adulteration we have to notice was discovered by M. Chevallier of Paris. The adulterating substance was stearine, one of the component principles of most fatty matters, the crystalline form of which resembles very nearly that of the sulphate of kina; and the proportion in the article was eight per cent. M. Chevallier suspected the presence of some foreign substance in consequence of the taste being not only bitter, but likewise acrid, and irritating to the throat; and he subsequently found, that, when gently heated in a silver spoon,

the crystals yielded a fluid which concreted by cooling, and was evidently greasy. He afterwards found that this portion possessed all the properties of stearine. The method he recommends for detecting this adulteration is a very simple one. Diluted sulphuric acid rapidly dissolves the sulphate of kina, and leaves the stearine, which may be collected in a concrete cake by heating the mixture short of the boiling point of water, and then allowing it to cool, when the stearine will concrete on the surface. This process will also detect other adulterations with some acicular crystalline bodies of the animal and vegetable kingdoms. There is a very easy kind of adulteration, however, which it will not detect,—namely, that with the acicular crystals of sulphate of lime formed from an acidulated solution of that salt. This is easily detected by the residue left after incineration. The sulphate of kina is of course wholly dissipated by incineration.

MEDICINE.

On Scurvy as it appeared among the British Troops in the Burmese War.—(*Circulars of the Med. and Phys. Soc. of Calcutta.*)—We have just received several monthly circulars giving an analysis of the proceedings of the Medical and Physical Society of Calcutta for August, September, October, and November last. They bear ample testimony to the activity of its members, and to the spirit of scientific inquiry which the society, if it has not implanted, has at all events evidently fostered and greatly increased among the medical officers of the East. Of the papers which have been lately read the most interesting are upon *Hospital Gangrene* as it appeared among the native artillerymen attached to the Nurbudda field force, by Dr Adam,—on the *Habitats of the various species of Rhubarb*, by Mr Boyle,—on an alleged *Specific for Cholera*, lately discovered by the Moravians, by Colonel Hazlewood,—on the *Diseases of the Army in the Burmese War*, by Dr Waddell,—on the *Medical Topography of Arracan*, and the diseases among the European troops stationed there in 1825, by Mr Stevenson,—on *Phthisis Pulmonalis*, as it appears in Hindostan, by Mr Bird. It would be an injustice to the authors of these papers to give any thing less than the short analysis of them which has been drawn up in the circulars before us; and our space does not permit us to transfer the analysis to our pages. But we hope to bring these subjects before the British reader in greater detail at a subsequent period, when we review the Society's Transactions. Meanwhile, we may extract from the analysis of Dr Waddell's paper the following account of *Scurvy*, a disease now rarely met with in our army or naval service, and which, it is not perhaps generally known, prevailed to an alarming extent among our troops soon after the invasion of Ava.

Scurvy appeared first prominently among the troops in September. To the common and well known symptoms of the disease were superadded in many instances diarrhoea and œdematous swellings of the feet; and a scorbutic diathesis, up to the month of February, prevailed in almost every case of dysentery. In the natives, this form of the malady was less remarkable, while the debility and hydropic symptoms were more striking. Whenever the feet became œdematous, certain death was the consequence, however apparently mild the other symptoms might be. Of the causes of the scorbutic diathesis, the nature of the salted provisions, the want of fresh vegetables, and the length of time, from the activity of the enemy, which they were compelled to subsist on the former, together with the peculiarly severe and harassing duty to which the troops were exposed, are enumerated as the principal; and, in the author's opinion, it was only remarkable that under such circumstances the malady did not sooner make its appearance. It is in his experience to have known the disease break out in an H. C. ship, among recruits not three months after leaving England, provided too with potatoes, and abundance of prophylactic and corrigent means. Yet at Rangoon, the European troops subsisted on salt meat alone, for a period of 7½ months, before scurvy was clearly apparent. To the native troops the provisions supplied were of excellent quality; and putrescent meat could not have any share in the production of the disease among them; they did not enjoy the pot-herbs which are so much in use in their native country; and to this, and the nature of their duties, is Dr W. disposed to attribute the occurrence of the diathesis. For the cure of the malady, the nitrous vinegar of Dr Paterson was first resorted to in the Europeans: where dysenteric symptoms did not exist, it appeared to answer completely. When anti-scorbutics were liberally supplied, such as Hodgson's ale, spruce beer, lime juice, &c. the disorder appeared stationary. Together with these

were administered the carbonate of magnesia, and rhubarb in small doses, with opiates and anodyne glysters. In the native subjects, the disease proved very untractable, and their recovery was more to be attributed to the amelioration of diet, and the change from a hot and moist to a cool and dry season, than to any of the remedies employed. Where death took place in the hydropic cases, it was attended with all the symptoms of hydrothorax.

At the meeting of the society on the 14th of October last, Sir James M'Grigor, and Dr Duncan Junior were unanimously elected Honorary Members.

Cases of the employment of the Alcoholic Extract of Nux Vomica in the cure of Rheumatic Paralysis and in the Incontinence of Urine of Children. (*Archives Générales de Médecine, Mars 1827.*)—A young man, an amanuensis, was suddenly attacked with difficulty in moving the right arm, and presently also with pain. When M. Mauricet, his physician, first saw him ten days afterwards, the shoulder and arm were very tender and swollen, and the fore-arm was always kept in a half-bent position, and in a state of pronation. Anodyne embrocations removed the pain in the course of eight days; but the patient could not execute the movements of the shoulder-joint at all. Half a grain of the alcoholic extract of nux vomica was then given twice a-day in the form of pill. The movements of the joint soon afterwards gradually returned. At first they were involuntary and limited in extent; but by and bye they became much more free, and the patient acquired the command of them. After sixteen days of this treatment, during which the pills were increased to four daily, the joint was restored completely to its natural state.

Two boys, one thirteen, the other fourteen years of age, had been liable from their infancy to nocturnal incontinence of urine. M. Mauricet, who was consulted by their father, resolved to try the effect of the extract of nux vomica, and gave each of them half-a-grain night and morning. In three days the incontinence disappeared, and it did not return so long as the remedy was taken. In fifteen days the remedy was discontinued, but the malady returned soon afterwards. The nux vomica was resumed, and with the same success. It was again omitted, and the complaint again returned. At last the extract being again resumed, and taken without intermission for a month, the children were radically cured.

We have noticed the foregoing cases with the view of calling the attention of practitioners to this remedy, which has not yet received in Britain the regard to which we conceive it is entitled. The risk alleged by some to attend its use on account of its poisonous properties appears visionary, when it is considered that it has not the power possessed by some poisons of accumulating in the body, and that its effects may be regulated to the utmost nicety by attention to the dose.

SURGERY.

Case of Encysted Tumour of the Mamma.—The following account of a case of compound encysted tumour of the mamma has been recently transmitted to us by Mr David Laing, Surgeon at Thurso. It forms an interesting addition to the cases of the same nature related by Dr Cumin of Glasgow in his Essay on Diseases of the Mamma in our last number.

"C. D. a stout unmarried woman, 29 years old, of a bilious temperament, applied to me in the month of August last with her right breast greatly enlarged. Three distinct clusters of hardened glandular bodies could be felt deeply situated in the mamma, one larger than a hen's egg, the other two smaller. The superficial veins were varicose, but the skin was not discoloured, nor were the axillary glands hard or enlarged. No diseased adhesions of the mamma to the chest seemed to have taken place. The patient described the pain, which at times was very severe, as sharp and lancing, producing a sensation as if needles were pushed into her breast. She noticed the tumour first about the end of 1824, since which period it had gradually attained the size and appearance which it had when I first saw her. The breast was then three times the size of the other, and measured twenty inches in circumference. The patient enjoyed tolerable health, and the catamenia were regular.

"Magendie had some time ago remarked the power which iodine possesses of reducing the size of the female breast, and as experiments in Britain, showing its power in discussing enlarged or hardened glands, had since tended to confirm that opinion, I considered this a favourable case for its exhibition. I therefore ordered it for her, beginning with twelve drops of the tincture twice a-day. It was also up-

plied in combination with the *tinctura saponis* as a liniment twice a-day to the breast, which was ordered to be well supported by a suspensory bandage from the shoulders. The patient residing at a considerable distance from me, I had not an opportunity of seeing her so often as I wished. Three weeks afterwards the tincture of iodine was given to the extent of fifteen drops three times a-day, and in nearly three months after the first exhibition of the medicine a decided improvement had taken place. The breast was much reduced in size, being nearly three inches less in circumference; the diseased clusters of glands seemed not so hard as when I first saw her; the pain was also greatly mitigated; and so much better did she consider herself, that she had engaged to work as servant on a farm. The disease appeared evidently to have been affected by the medicine, and the case altogether appeared to be doing so well that I anticipated a favourable result. About the end of December, however, the patient presented herself to me much worse, with all the symptoms greatly aggravated; the breast had increased in size; the pain, particularly at night, was excruciating and extended to the axilla; and her general health was not nearly so good as formerly. About the time when the symptoms became worse she had been a good deal exposed to fatigue in working. I then considered the disease as rapidly proceeding to cancer, and decided upon extirpation of the breast as the only chance for the patient. This was done in the usual way on the 2d January last. The pectoral muscle was laid bare, and the mamma completely dissected away.

“Immediately after the operation the gentlemen with me, upon examining and cutting into the diseased parts, were much surprised, when the largest mass was laid open, to see a considerable quantity of thin fluid of a very dark colour spring out. The fluid was contained in a distinct sac, along with the hardened glands, which were of a dark gray colour. The other two hardened clusters of glands, with a few smaller ones, presented a similar appearance; all of them were contained in distinct sacs, along with a dark-coloured fluid. I regret exceedingly that, owing to the escape of the fluid among blood and water, it was not submitted to analysis. The diseased glands lay deep in the mamma, close upon the pectoral muscle, and when cut through showed small white lines shooting in every direction. The wound has some time ago completely healed up. The patient enjoys good health, and does not complain of any uneasiness in the axilla.”—*Thurso, May 22, 1826.*

Employment of Galvanism in the Reduction of Strangulated Hernia and Internal Strangulations. (*Archives Gén. de Méd.* Octobre 1826.) The following curious experiments are well worthy of the attention of surgeons, although the practical application of which they are susceptible has not yet been made. It has been found that when one pole of a small galvanic battery is connected with the mouth, and the other with the anus, the intestines are thrown into increased action, and in a few minutes one or more alvine evacuations are caused. Reasoning apparently from this fact, M. Le Roy d'Etiolles was led to conceive that the topical application of galvanism might be of use for effecting the reduction of strangulated hernia, when other means could not be employed on account of their slow action, or the irritation they produce, or the increase of the fecal matter which they are apt to cause in the strangulated portion by stimulating the superior part of the gut. He accordingly proceeded to try the accuracy of the idea so far as it could be determined by experiments on animals. He first, however, repeated upon himself the experiment already mentioned with the poles of the battery in the mouth and anus; and he found that, with a trough-battery of twelve pairs of inch-and-half plates, he felt a sense of slight heat at each point of contact, faint flashes of light, movements in the bowels without any general shock, and in a quarter of an hour a sense of weight in the rectum, followed by one or two evacuations. Any considerable augmentation of the number of plates produced so strong a sensation of heat that he could not endure the contact of the wires except for an instant; but after repeated shocks communicated in this way, the bowels were moved in fifteen minutes with nearly the same antecedent phenomena, and without any injury to the health.

He next drew out a loop of intestine from the belly of a rabbit through an incision in the parietes, and repeated the experiment just described with five or six pairs of plates. The portion of intestine, which had been previously immovable, immediately exhibited a brisk undulatory motion, without any general shock or contraction of the muscles of the parietes; and the motion continued some time after the galvanic circle was interrupted. If a ligature was applied round the protruded gut,

the movements were less brisk, but still obvious, and they evidently tended to free the gut from the ligature. If the conductor, which in the foregoing experiments had been placed in the mouth, was applied to the protruded gut, the gut immediately contracted at the point of contact; and if both conductors were applied to the protruded gut, the contraction which took place was much greater, and the fecal matter was pushed forcibly towards the superior and inferior parts of the intestines. When this effect was produced on several successive portions of the protruded gut, its contents were gradually discharged upwards and downwards, and its diameter was very much diminished. When the protruded gut, containing fecal matter and gases, was tied with a ligature to imitate strangulated hernia, the foregoing operation soon reduced its diameter to a third or a fifth of what it was originally, so that the gut could be easily pushed through the ligature. When the ligature was applied so as to compress only a half or a third part of the circumference of the gut, the strangulated portion, after the action of galvanism, receded spontaneously. The very same results were procured when the integuments were brought over the protruded gut, and the galvanic fluid was transmitted to it through acupuncture needles. When the gut was tied with a ligature, and then returned into the cavity of the abdomen, the portion operated on was found sensible to galvanism twelve hours afterwards.

The author infers from these experiments, that in volvulus and some other varieties of internal intestinal obstruction, the obstruction may be removed by the brisk peristaltic action which galvanism excites throughout the whole intestines when transmitted from the mouth to the anus; and that in like manner some forms of strangulated hernia, particularly those in which the reduction is prevented by the accumulation of fecal matter, may be brought by the application of galvanism through acupuncture needles into such a state as to admit of easy reduction by the *taxis*.

On the Treatment of Drowning and other varieties of Asphyxia. (*Archives Générales de Médecine*, Novembre 1826.)—Some experiments and observations of considerable importance have lately been made at Paris on the circumstances which modify the effects of two of the chief remedies for asphyxia,—namely, insufflation of the lungs and galvanism. The author, M. Leroy d'Etiolles, has succeeded in throwing some light on the variable results which have been procured by these modes of treatment by almost every person who has employed them.

With regard to Artificial Respiration, he has found that it requires much more cautious management than is generally believed; for if the air be blown into the lungs with too great force, they become unfit for their function. This he says he has found by repeated experiments on rabbits, dogs, and sheep. The following results were procured with the sheep, an animal whose lungs approach nearest in point of volume to those of man. An incision being made into the windpipe, and a silver tube secured in it by a ligature, a single forcible inspiration was made through the tube with the mouth, and the tube and ligature were immediately withdrawn. In the course of a single minute the animal became agitated and restless, made forcible efforts to inspire and expire, was then seized with convulsions, became motionless and senseless, and the pulse ceased in three or five minutes. If the carotid artery was opened during these symptoms, the arterial blood was observed to become rapidly more and more dark and venous. These phenomena were remarked in four successive experiments. In other three made with the same animal under the same circumstances, except that the insufflation was performed less rapidly, the breathing was not arrested so suddenly, but became at first embarrassed, and gradually more and more so, till at length death took place in about fifteen minutes from the beginning of the experiment. The symptoms under which these animals died clearly indicated death by asphyxia, and the appearances in the dead body were conformable. The cause of the production of asphyxia under forcible insufflation is not very apparent; at least M. Leroy has not hitherto succeeded in throwing any light upon it. In the second variety of cases, the air-cells of the lungs were probably ruptured, and a communication between them and the cavity of the chest even established through the pleura, for the lungs could not be dilated after death. But in the first variety, in which death took place immediately, the same injury was not produced. The practical conclusion, however, to be drawn from his experiments is manifest and important,—namely, that in performing artificial insufflation, the bellows ought never to be worked forcibly; and we may add another reason for

cautious management of them,—that the operator is not perhaps always aware how great the force is which he employs in working them. With the bellows a far greater force is easily put forth, than can ever be employed with the mouth, which was M. Leroy's instrument. If M. Leroy, however, is correct in considering the forcible expansion of the lungs as the cause of death in his experiments, the objection which applies to the forcible use of the bellows in the human subject, does not apply so pointedly to forcible insufflation with the mouth; because the operator subjects his own lungs to the same pressure as those of his patient, so that his own lungs will enact in a manner the part of a safety-valve, provided he is not superior in strength of chest to his patient.

The other topic which forms the subject of M. Leroy's remarks is the application of Galvanism to the treatment of asphyxia. It is well known, that, although this remedy has been sometimes found singularly useful, it has very often failed, and not unfrequently it has even appeared to do harm. M. Leroy thinks he has discovered the cause of its failure. When the galvanic current is applied in such a way as to affect the diaphragm, it is almost always applied continuously, in which case the contractions are irregular, and do not constitute a natural inspiration. But if the current is suspended as soon as the muscular contraction begins, then a complete inspiration is produced, and the regularity of the subsequent expiration is not interrupted: on connecting the galvanic circle, a new contraction is produced, which in like manner causes a natural inspiration, if the circle is again interrupted. The plan recommended by M. Leroy, and adopted in his experiments, is to insert a fine acupuncture needle between the 8th and 9th ribs, till it reaches the attachments of the diaphragm, for which purpose a length of a few lines is sufficient,—and then alternately to complete and interrupt the galvanic circle, by touching the needle with one conductor at the proper intervals, while the other is kept permanently in the mouth.

On the treatment of Poisoned Wounds by ligatures round the limb nearer the heart. (*Archives Générales de Médecine, Novembre 1826.*)—M. Bouillaud has made some farther experiments on this subject; and they tend to confirm what is now pretty well established, that a ligature applied between the heart and a wound into which a poison has been introduced will prevent or suspend its deleterious effects. His experiments were made with strychnia. The only novelties in them are the proof that it may be applied to the extremity of a divided nerve, without any of its usual effects ensuing,—and a much more important result, namely, that by alternately relaxing and tightening the ligature at proper intervals, an animal, into which a fatal dose has been introduced, may be alternately affected and relieved, till the whole poison has been absorbed, and has exhausted its energy. The author does not seem to have been aware of this practical application of the facts he observed. It is evident, that, with regard to such poisons as opium, strychnia, and hydrocyanic acid, the system possesses the power either of discharging or decomposing them. Hence, even after they are beyond the reach of direct extraction, their effects may still be controlled by art, if the physician can succeed in admitting them into the circulating system by little and little; for each successive portion is decomposed, and the poison has not an opportunity to accumulate in the blood to a sufficient extent to prove fatal. These views result from the experiments of M. Bouillaud now mentioned, and likewise from those of Dr Barry on the effects of dry cupping, when applied to wounds poisoned with the active vegetable poisons.

Anomalous state of the Pulse in Strangulated Hernia.—(*Extract from a letter to the Editor from William Law, Esq.*)—"Circumstances prevent the following being given in detail; but as the lesson to be derived is independent of this, the true purpose will be served by the leading features, which are these. It was a hernia, presenting the singular anomaly, that, during four days continuance of strangulation, the pulse scarcely rose at any time above 60 in the minute, or so high, and was full in the beat, not thready, notwithstanding the presence of nausea and much vomiting. The rupture was complicated with other disease of the parts, and various circumstances concurred to throw obscurity over the case, so as to conceal its real nature from several surgeons who had seen and operated on many herniæ. When the operation was finally resorted to, the incarcerated part of intestine and caul had still not reached any irremediable state of inflammation, being found tolerably healthy at the opening of the body after death also, which appeared to have ensued

from sinking. While I remark the absence of a usual important diagnostic symptom in this case, it should be known that the ordinary state of this person's pulse, when in health, was not in point of frequency below the common standard; in regard to strength it was greater than is generally met with."

LEGAL MEDICINE.

On Poisoning with the Sulphurets of Arsenic. (*Journal de Chimie Médicale et Journal de Pharmacie, passim.*) Several memoirs have lately appeared in the Parisian Journals on the subject of poisoning with the sulphurets of arsenic; and as they explain some facts which have long appeared unintelligible, we shall present a condensed view of the whole. Till very lately it was believed, that, as the artificial sublimed sulphurets are much more poisonous than those of natural origin, some essential difference existed between them in chemical composition. M. Guibourt, however, has completely shown the cause of the differences in their physiological effects. For he has found that the artificial sulphurets always contain a large proportion of oxide of arsenic, sometimes so much as 96 per cent. This observation has been more recently confirmed by M. Courdemanche; and we may add that we have seen the cakes of sublimed orpiment lined on their concave surface with octaëdral crystals of the oxide. M. Guibourt, on the presumption arising from this difference, and from the known harmlessness of arsenic in its metallic state, ventured to hint, that the proper sulphurets, such as the native sulphurets, and that procured by transmitting sulphuretted-hydrogen through a solution of the oxide, are not deleterious. But Orfila has contradicted this statement by positive experiments. He found that when 40, 60, or 120 grains of the pure artificial sulphuret procured by precipitation, or of the native orpiment of Hungary, or the native realgar of Transylvania, were introduced into the stomach, or into the cellular tissue of dogs, death took place in two, four, or six days, under the usual symptoms of poisoning with arsenic. It is certain, therefore, that the sulphurets of arsenic, when perfectly pure, are poisonous; but they are much less so than the oxide. Their poisonous power appeared a remarkable and anomalous fact, considering that metallic arsenic has been proved not to be poisonous in its pure state, or when alloyed with tin, or when combined with iron and sulphur in the ore mispickel or arsenical pyrites. But the anomaly has been accounted for by M. Courdemanche, who finds, (*Jour. de Pharm. Mai, 1827.*) that if the pure sulphuret of arsenic be boiled even in distilled water, a part of the water is decomposed, sulphuretted-hydrogen is evolved, and the white oxide of arsenic is formed and remains in solution,—that this change takes place in the cold, but much more slowly,—and that it is very much accelerated by the presence of vegetable or animal principles in the water. In all likelihood, then, the sulphurets are poisonous merely because they are in part converted into the oxide. These observations will also account for the statements of Hahnemann regarding the solubility of the sulphurets of arsenic in water. They are insoluble as sulphurets, but will lose weight by boiling, because they are decomposed.

Poisoning by Phosphorus.—(*Mém. de la Soc. Méd. d'Emulation, Tom. IX.*) On the 24th April 1824, a young man, 28 years of age, swallowed half a grain of phosphorus mixed with very hot water. Feeling no effect, he took three days after a grain and a half in the same vehicle at a single dose. He breakfasted almost immediately after, and experienced no remarkable symptom till five, when he had no sooner swallowed some food than he complained of violent pains of the stomach and belly. These were soon followed by incessant and painful vomiting, and abundant discharges from the bowels during the night. On the following day these symptoms were aggravated by violent contractions of the belly. Emollient injections produced neither excretion nor relief. The patient, however, followed his ordinary pursuits; and it was only on the 4th of May (after a full week) that he communicated to M. Worbe the circumstances of his complaints. The epigastrium was then extremely painful to the touch, the abdomen was very tense, and its muscles strongly contracted; and he was unable to continue out of the horizontal posture. The features had an expression of sadness, languor, and wavering; the eyes were heavy, and the conjunctiva and skin of a strong yellow tint. The respiration, however, was natural, the pulse was only a little hard, and the tongue and mucous membrane of the mouth were unchanged. *Membra generationi inservientia nequaquam affecta.* The remedies were leeches to the epigastric region, the warm-bath, fomentations, and emollient cataplasms, mucilaginous injections, and for drink gum-solution in abun-

danced. The leeches were not applied till noon. At ten in the evening the patient was delirious, convulsed, and unmanageable, but carried his hands instinctively to the epigastric region. A physician summoned at seven the next morning ordered to each elbow fifteen leeches, which procured a good deal of blood. M. Flourens, introduced by M. Worbe, recommended leeches to the head; and they were accordingly applied. The condition of the patient, however, became hourly worse. The urine escaped involuntarily; the alvine discharges became frequent and copious, with flabbiness of the muscles; the beats of the heart became weak, and those at the wrist were no longer perceptible. The surface, which was now intensely yellow, became covered with a cold sweat, which was most abundant on the forehead; the extremities became cold; and death took place at three in the morning of the 6th, the second day of his application to M. Worbe.

The external appearances after death were, yellow tint of the skin, the cutaneous veins of the belly and upper part of the thigh prominent, and the skin of these parts checkered with livid spots, sugillations, and venous impressions. The scrotum was bluish and phosphorescent, according to the observation of M. Bogros. The chest contained much blackish serous fluid; the lungs were gorged with blood; the heart was soft, collapsed, and contained little blood. The muscular tissue only of the stomach was inflamed; the other membranes of the stomach and duodenum were pale and flaccid, and gas was developed in their submucous cellular tissue. The cardiac and pyloric orifices presented black or rather slate-coloured patches, which might be compared to the blood-shot spots seen on the conjunctiva. All the intestines were tympanitic, but contained little fluid. The bladder was sound, and contained four ounces of urine. Permission to examine the brain could not be obtained.

M. Worbe and M. Bogros killed two dogs, one of 7, the other of 18 pounds weight, by giving the first half a grain, and the second a grain and a half of phosphorus dissolved in hot water. In the former dog the cardia and pylorus presented the black spots observed in the stomach of the subject above-mentioned, but the brain and its appendages were sound. In the second several ecchymosed spots were found in the cellular fat, contiguous to the base of the ventricles of the heart, and the surface of the auricles. The villous membrane of the alimentary canal, especially that of the stomach, was folded or contracted, and easily detached; and the tube was abundantly filled with blackish matter, as far as the cæcum. The veins of the brain were filled with black blood; the membranes were injected, and the pia mater was ecchymosed.

London Bills of Mortality from December 13, 1825, to December 12, 1826.—[*Morning Chronicle*, December 28, 1826.]—CHRISTENED in the ninety-seven parishes within the walls, 913; buried, 1318. Christened in the seventeen parishes without the walls, 5059; buried, 4148. Christened in the twenty-nine out-parishes in Middlesex and Surrey, 12,604; buried, 11,423. Christened in the ten parishes in the city and liberties of Westminster, 3677; buried, 3869. Total christened, 22,244; buried, 20,758.

Diseases and Casualties.—Abscess 76, Age and Debility 1575, Apoplexy 363, Asthma 922, Bedridden 14, Bile 14, Cancer 100, Consumption 5290, Contraction of the Heart 2, Convulsions 2588, Croup 90, Diarrhœa 12, Dropsy 820, Dropsy of the Brain 676, Dropsy of the Chest 65, Dysentery 7, Enlargement of the heart 16, Epilepsy 49, Eruptive Diseases 12, Erysipelas 17, Fever 926, Typhus Fever 97, Intermittent Fever 2, Flux 7, Gout 38, Hæmorrhage 34, Hernia 28, Hooping-cough 674, Hydrophobia 4, Inflammation 2295, Inflammation of the Liver 119, Insanity 179, Jaundice 33, Lock Jaw 1, Measles 774, Miscarriage 3, Mortification 244, Ossification of the Heart 6, Palpitation of the Heart 6, Palsy 22, Paralytic 125, Pleurisy 13, Rheumatism 12, Scrophula 10, Small-Pox 503, Sore Throat 13, Spasm 44, Still-Born 733, Stone 21, Stoppage in the Stomach 20, Sudden Death 110, Teething 309, Thrush 65, Tumour 8, Venereal Disease 8, Worms, 1. Total of diseases 20,374.

Casualties.—Burnt 28, Choked 1, Drowned 139, Excessive Drinking 7, Executed 2, Found Dead 9, Fractures 2, Fright 1, Falls and other accidents 112, Murdered 4, Poisoned 8, Scalded 2, Shot 1, Smothered 1, Starved 2, Suffocated 8, Suicides 57. Total of Casualties 384.

Of those Christened 11,178 were males, and 11,066 females. Of the deaths 10,454 were males, and 10,304 females. Of the deaths 5952 were under two years of age, 1982 between two and five, 768 between five and ten, 808 between ten

and twenty, 1472 between twenty and thirty, 1724 between thirty and forty, 1994 between forty and fifty, 1926 between fifty and sixty, 1832 between sixty and seventy, 1569 between seventy and eighty, 634 between eighty and ninety, 90 between ninety and a hundred, one was a hundred years old, 3 a hundred and three, 3 a hundred and five. The burials were 268 fewer than in 1825.

Statistical Account of the Lunatic Asylum at Aversa in the kingdom of Naples. (*Archives Générales de Médecine, Octobre 1826.*)—M. Esquirol has published from documents supplied by Dr Latrillo, one of the resident physicians of the Lunatic Asylum at Aversa, an interesting account of many particulars relative to the Statistics of Insanity as it prevails in the kingdom of Naples. The documents embrace a period of ten years, from 1814 to 1823 inclusive. The following are the principal results at which M. Esquirol has arrived on comparing the reports of Aversa with those of other European asylums.

The number admitted in ten years was 1725, or $172\frac{1}{2}$ annually, which is a much less proportion to the population than is observed in France and England. The males were to the females in point of number as five to two: in France the proportion of the sexes is nearly equal: in St Petersburg and Moscow it is as five to four. These facts show that it is not the climate which renders insanity less frequent among females in Naples, but their inferior importance in society, and consequently a less frequent exposure to the moral causes of the disease. The season during which the admissions have been most frequent is midsummer, and the months immediately preceding and immediately following, namely, May, June, July, and August. This result accords with what has been observed in France and England. Among the causes the first noticed is hereditary tendency. This appears, however, to be a very rare cause in Naples, for, if the records are correct, only 20 cases of the 1725 were cases of hereditary insanity. Fever is a very common cause in that country. Love has been the cause of about a twelfth part of the cases, being a much larger proportion than in France. "It is long," says Esquirol, "since love has ceased to break hearts and turn heads in France." Of the various forms of insanity *Monomania* has been by far the most frequent. Eight hundred and forty-three were of that description. The Cures at Aversa have been three-tenths of the males affected, five-seventeenths of the females. This proportion is not so favourable as in the chief asylums of France and England. At the Salpêtrière and Bicêtre the cures amount to two-fifths of the whole. The greatest number of cures were affected at Aversa, as in France, during the months of September, October, November, and April. The Relapses have been 92, and were most frequent among the male patients. The latter fact accords with Esquirol's own experience; but in Paris the relapses are not so frequent. The mortality has been very high at Aversa,—being no less than a full fourth of the whole. At Paris it is only a thirteenth. The greatest proportion of deaths has been in the months of November, December, and January. The causes of death have been chiefly exanthematic fevers, dry gangrene, rabies (?), very rarely palsy, which is the cause of one half of the deaths in Paris. In 1816 many of the patients were carried off by typhus,—a new proof of the incorrectness of the old idea, that insane people can resist exposure to the causes which produce ordinary disorders in the sane.

Medical Graduations at Edinburgh.—On the 1st of August 1826, the Senatus Academicus of the University of Edinburgh conferred the degree of Doctor in Medicine on the following Gentlemen, in number one hundred and nineteen, after having gone through the appointed examinations, and defended publicly their inaugural dissertations.—(*This list was accidentally omitted in the Number for last October.*)

Of Scotland.

Alexander Bertram,	De Asthmate.	John Caw,	— Dyspepsia.
Charles Robert Boyes,	— Rubeola.	Duncan M. Church,	— Dyspepsia.
Alexander Bryce,	— Respiratione.	John Clark,	— Variola.
Alexander Calman,	— Pertussi.	David Ross Cundell,	— Hæmorrhagia
Archibald Campbell,	— Cynanche		Uteri Puerpera.
	Tracheali.	Robert Dickson,	— Phthisi Pul-
John Shaw Campbell,	— Febre Puer-		monali.
	perali.	Henry Douglas,	— Pulsu.
John A. Carlyle,	— Mentis	Andrew V. Dunlop,	— Cataracta.
	Alienatione.	James Eaton,	— Hepatitide
			Acuta.

James Ford, . . .	De Epilepsia.	William Tabrum, . .	De Chlorosi.
James Gardner, . .	— Paralyseos	William Wallace, . .	— Diabete.
Historia, Pathologia, Causisque.		William B. Wilmot, . .	— Epilepsia.
James Gibson, . .	— Dysenteria	Thomas Woodforde, . .	— Mania.
	Tropica Acuta.	<i>From Wales.</i>	
George A. Gordon	— Iritide.	Thomas Evans, . .	— Pneumonia.
George Hay, . .	— Enteritide.	<i>From Ireland.</i>	
Thomas Hay, . .	— Vi Caloris in	Thomas Ahern, . .	— Dysenteria.
	Corpore Humano.	William Armstrong, . .	— Febre Puer-
Murdo Mackenzie,	— Rabie Canina		perali.
Kenneth Mackinnon,	— Osse Secer-	Gerard P. Barry, . .	— Gastritide.
	nendo.	William Barry, . .	— Phthisi Pul-
James Martin, . .	— Neuralgia.		monali.
James Murray, . .	— Placenta.	Patrick Boyle, . .	— Febre Flava.
John S. Muschet, . .	— Apoplexia.	Edward G. Bruncker, . .	— Apoplexia
James Paton, . .	— Rubcola		Sanguinea.
Daniel Reid, . .	— Epilepsia.	Richard Burke, . .	— Hepatitide.
Mark Richardson, . .	— Rheumatis-	Thomas Burrows, . .	— Ascite.
	mo.	Patrick Byrne, . .	— Tetano.
Thomas G. Stephenson, . .	— Cholera Mor-	John Carey, . .	— Veneno Arse-
	bo.		nicali.
George Wilkie, . .	— Rheumatismo.	Thomas Chapman, . .	— Hæmorrhæa
John Wilson, . .	— Syphilide.		Petechiali.
<i>From England.</i>		Patrick Connor, . .	— Sanguinis in
Joseph B. Anderson, . .	— Ascite.		Typho mittendi Utilitate.
James P. W. Baker, . .	— Pubertate.	William Donnelly, . .	— Usu Intem-
Joseph Blagborne, . .	— Paralyti.		pestivo quorundam Remedium.
Thomas Blundell, . .	— Melanosi.	James Ferguson, . .	— Hæmoptysi.
Watkin L. Collins, . .	— Vitiis Organi-	James Brown Gibson, . .	— Enteritide.
	cis Cordis.	Roger Green, . .	— Catamenii.
James Eaton, . .	— Pleuræ In-	Samuel Thomas Gregg, . .	— Asthmate.
	flammatione.	William Griffin, . .	— Dolore.
Klein Grant, . .	— Inflammatione	John Haire, . .	— Phthisi Pul-
	Medullæ Spinalis		monali.
Charles C. Greville, . .	— Hydrothorace.	William Hare, . .	— Dysenteria.
David James Hall, . .	— Phthisi.	George Hartford, . .	— Pathologia
Arthur Hedley, . .	— Operatione		Fulmonis.
	Cæsareana.	William Keily, . .	— Apoplexia
H. F. W. Heydeloff, . .	— Catamenii.		Sanguinea.
	Morbisque Uteri.	John Brennon Kilbride, . .	— Rheumatis-
Algernon Hicks, . .	— Concoctione.		mo.
Richard James, . .	— Causis Motus	Timothy MacCarthy, . .	— Vermibus In-
	Sanguinis.		testinorum.
George Kirk, . .	— Febribus In-	Ross MacClintock, . .	— Hæmorrhagia
	termittentibus.		Uterina.
Thomas Lloyd, . .	— Colica Picto-	William Mackay, . .	— Febre Flava.
	num.	Andrew M. Macmoran, . .	— Pneumonia.
Edward Lubbock, . .	— Venis Varico-	Dionysius J. Magrath, . .	— Hypochon-
	sis Cruris.		driasi.
Robert Nairne, . .	— Iritide.	William Murray, . .	— Ascite.
Francis Richard Philp	— Clavo Secali.	John Murtagh, . .	— Enteritide.
Thomas Raffington, . .	— Pathologia	John Nolan, . .	— Usu Aquæ in
	Hæmorrhagiæ.		Inflammatione.
William Robinson, . .	— Phlegmasia	John Nalty, . .	— Membranis
	Dolente.		Mucosis.
Richard Roscoe, . .	— Rheumatismo	James Paton, . .	— Pneumonia.
	Acuto.	Thomas Paye, . .	— Injuriis San-
John Smith, . .	— Historia Chi-		guinis Detractione Ortis.
	urgiciæ.	William F. Porter, . .	— Dysenteria.
James Stokes, . .	— Vulneribus in	Samuel L. Potter, . .	— Enteritide.
	Sectione Cadaverum acceptis.	Thomas Robertson, . .	— Embryoto-
Sigismund Stolterfoth, . .	— Nervorum		mia.
	Systemate.	Thomas Scully, . .	— Enteritide.

Patrick Sheehan, . — Rheumatismo Acuto.	G. R. Stennett, . — Febribus Indis Occidentalis.
A. E. Tabuteau, . — Usu Externo	From Nevus.
Atropæ Belladonnæ.	Thomas Armstrong, . — Respiratione.
John Tully, . — Hepatitide Acuta.	From Berberice.
Richard Tuohill, . — Hydrothorace.	Samuel B. Beresford, . — Colica Indica, seu Plumbea.
Richard Tuthill, . — Pertussi.	John B. Beresford, . — Trismo Nascentium.
Charles Vaughan, . — Dysenteria.	From Nova Scotia.
James Blackall Wall, . — Hæmorrhagia Uterina.	Samuel Bayard, . — Pertussi.
From Guernsey.	From Canada.
Henry Curtis, . — Hydrope.	John Jenkin Maclean, . — Ascite.
From Barbadoes.	From America.
Barclay Harper, . — Phthisi Pulmonali.	John Burton, . . — Pneumonia.
Edward Skeete, . — Podagra.	William Prince Wilson, — Membranis Mucosis.
From Jamaica.	From E. Indies.
T. W. Anderson, . — Hydrope.	Allan Gilmore, . — Dysenteria.

Extract of a Letter from Professor Scarpa to Mr Wishart of Edinburgh.—I feel much gratified by your congratulation with regard to the prolongation of my life, which some journalists had thought proper to cut short.

I have been for nearly two years acquainted with Mr Wardrop's splendid and fortunate operation of the ligature of the carotid aneurism seated deep under the sternum, and I am much pleased to hear from you that some other similar operations have been performed with equally good success. We must allow that this mode of operating is not applicable to all cases of aneurism, but only to that just mentioned, in which there is opposite to the origin of the carotid a large artery almost of the same size as the carotid. And it is a fact, that this mode of operating is not at all applicable to the case of inguinal aneurism, because the *profunda* femoral artery is of a calibre much less than the *external* femoral artery. At all events, this discovery of Mr Wardrop is valuable for the advancement of the art; and I regret that it was omitted in my work, although I entertained the hope that the blank might be filled up in a new edition of it, which is now publishing in Paris.

In the course of a month a new work of mine will be published here under the title *De Anatome et Pathologia Ossium Commentarii*, with six plates. In this work I propose to review what I have already stated at a former time, with regard to the true structure of bone, and to rectify and arrange in better order the most certain and important facts with regard to the *Essence* of the Diseases of the bones, and the artificial manner employed by nature in the formation of callus for the reunion of fractured bones, in all which subjects there is a want of precision in the doctrines of surgery.

Along with the books I am to send to you, you will receive a small work, from which you will perceive that we are acquainted with the possibility of arresting the progress of the incipient *medullary fungus* of children, by means of the extirpation of the eyeball, which has been hitherto a subject of controversy. You will also find a memoir on a rare and perhaps unique case of *diaphragmatic hernia* on the right side of the diaphragm provided with a hernial sac, a very rare circumstance.

Pavia, May 21, 1827.

THE
EDINBURGH
MEDICAL AND SURGICAL JOURNAL.

No. XCIII.

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The following works have come to hand :—

Lectures on the Operative Surgery of the Eye ; or an historical and critical Inquiry into the methods recommended for the cure of cataract, for the formation of an artificial pupil, &c. &c. &c., containing a new method of operating for cataract by extraction, &c. &c. By G. J. Guthrie Esq. 2d edit. with seven explanatory plates. London, Printed for Burgess and Hill. 8vo. 1827.

Medical Ethics, or a Code of Institutes and Precepts, adapted to the professional conduct of Physicians and Surgeons. By the late Thomas Percival, M. D. &c. &c. ; with additions illustrative of the past and present state of the Profession and its Collegiate Institutions in Great Britain. London, Published by W. Jackson, 1827.

Introduction to the Science of the Pulse, as applied to the practice of Medicine. By Julius Rucco, M. D., &c. &c. two volumes large 8vo. London, printed for the Author, and to be had of Burgess and Hill, &c. &c. 1827.

A Five Years' Residence in Buenos Ayres, during the years 1820 to 1825, &c. &c. By an Englishman, Second Edition. London, 1827.

Mems. Maxims, and Memoirs. By William Wadd, Esq. F. L. S., Surgeon Extraordinary to the King. London, Printed for Callow and Wilson, 1827, 8vo.

A Case Book for registering cases and occurrences in Medical and Surgical Practice, (useful to the Clinical Student and Practitioner.) Sold by W. Jackson, Medical Bookseller, London, 1827.

Dissertation sur le Passage du Sang à travers le Cœur, par David Barry, Docteur en Médecine. A Paris, 1827. 4to, pp. 21.

Observations on the Causes and Prevention of Fever and Pauperism in Ireland. By Richard Grattan, M. D., &c. Dublin, 1826.

Journal des Progres des Sciences et Institutions Medicales en Europe, en Amerique, &c. par une Association de Medecins, IVme volume, 1827. Paris, Villeret et Co. Libraires Editeurs, Rue de l'Ecole de Medecine, No. 13. 1827.

The Philadelphia Monthly Journal of Medicine and Surgery, Edited by N. R. Smith, M. D., Professor of Anatomy in Jefferson College, &c. Volume I. No. I. Philadelphia, Published by R. H. Small, No. 165 Chestnut Street, 1827.

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Magazin der ausländischen Literatur der gesammten Heilkunde, und Arbeiten des Aerztlichen Vereins zu Hamburg; herausgegeben von Dr G. H. Gerson und Dr Nikel. Hein. Julius, Marz, April, 1827.

A Dissertation on the remote and proximate causes of Phthisis Pulmonalis, to which the prize was adjudged for 1825, by the New York State Medical Society. By Andrew Hammersley, M. D., &c. &c. 2d Edition, pp. 99, 8vo. New York, 1827.

De Varioloidibus et Varicellis. Scripsit Nic. Christ. Möhl, Adj. Phys. Hafn. 12mo, Hafniæ, 1827, pp. 111.

The Philadelphia Journal of the Medical and Physical Sciences; new Series, No. 9. Philadelphia, Carey, Lea, and Parey, Chestnut Street, 1827.

The Carolina Journal of Medicine, Science, and Agriculture. New Series. Edited by Thomas Y. Simons, M. D. Fort Physician, &c. &c. Vol. 1. No. 2. May 1826.

Versuch einer kritischer Geschichte der bei vaccinirten beobachteten Menschenblättern nebst Untersuchungen über die Natur, die Ursachen, und die Verhütung dieser Krankheit von D. Adolph Friedrich Lüders, pp. 226. 8vo. Altona, 1824.

Vacciarum nativarum historia, cuncta, quæ de hoc morbo hucusque innotuerunt continens; Programma quo Facultatis Medicæ Kiliensis Decanatum susceptum indicit Adolphus Fredericus Lüders, M. D. et Prof. P. O. Inst. Clinici, pp. 29, Kiliæ, 1826.

C. A. Wilmann's Med. Stud. Bremani, Commentatio de Anthelminticis regni vegetabilis, quam in Certamine Literario civ. Acad. Georg. August die iv Junii A. MDCCCXXVI. proximo ad palmam accepisse Ordo Medicus declaravit, 4to, pp. 59. Gottingen, 1826.

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1. OCTOBER 1827.

PART I.
ORIGINAL COMMUNICATIONS.

ART. I.—*Observations on the Epidemic Fever now prevalent among the lower orders in Edinburgh.* By WILLIAM PULTENEY ALISON, M. D., F. R. S. E., F. R. C. Phys. Con-junct Professor of the Institutes of Medicine in the Uni-versity of Edinburgh, &c. &c.

I DO not propose to give in this paper a detailed history of the fever which has lately been, and still continues, though in a less degree, prevalent among the lower orders in Edinburgh. So many accurate and faithful descriptions of the disease have lately appeared in this country, that such a history, if I had leisure to undertake it, would be really superfluous. My object is to lay before the public only such observations, made during the epidemic, as seem to me of some value, in reference to certain points in the history and pathology of fever, on which a difference of opinion has lately prevailed, and the determination of which is of real practical importance.

This city has not for many years past enjoyed at any time such complete exemption from continued fever as would appear to have been the lot of some other large towns, particularly in England. Indeed, it must be owned that there are many circumstances in the situation and construction of a great part of the old town, and in the habits of a great part of its inhabitants, peculiarly favourable to the developement and propagation of fever. For about five years, however, before 1826 the amount of fever among the lower orders was certainly not beyond the

average. The ordinary fever wards in the hospital (comprising thirty-two beds) were generally sufficient to accommodate all the cases that presented themselves, and a few only were occasionally taken into the clinical wards. During my own attendance at these wards, in four clinical courses between 1822 and 1826, I had under my care in all only twenty-six cases of fever, one of which was fatal.

When I began to attend the clinical wards in February 1826, the number of fever cases presenting themselves was so great, that there was some difficulty in accommodating them, and it became necessary to admit an unusual number into the clinical wards. In order to diminish as much as possible the chance of the disease spreading by infection, one side of each of the two clinical wards then existing, (which contain in all forty beds,) was appropriated to fever patients. This arrangement lasted till August 1826, during which time ninety-six well-marked cases of fever came under my care. A large proportion of these were from Leith. It is worth notice, that in the men's ward there was no instance of any patient, admitted on account of other complaints, taking fever in the house, notwithstanding its continued presence on the opposite side of the ward. In the women's ward, two patients, admitted on account of other complaints, took fever, and a third recently dismissed returned ill of fever so soon, that her residence in the ward might reasonably be regarded as the cause. These three cases proved mild.

In November 1826, when, by the concurrent resolutions of the managers of the Infirmary and medical professors, all the four wards on the first floor of the hospital were appropriated to clinical instruction, Drs Home and Duncan set apart the smaller wards on each side (the old clinical wards) for fever cases only; and several other wards in the hospital were also filled with fever patients. But the numbers both from Edinburgh and Leith having greatly increased at that season, (as has always been the case during my observation of epidemic fever in Edinburgh,) the managers determined on again fitting up Queensberry House for fever patients, which was accordingly opened about the middle of December, and the duty of attendance there was undertaken by those of the clinical professors, who were not on duty in the Royal Infirmary, with the assistance of a resident physician, (Dr Reid.) Several of the wards in Queensberry House were therefore under my charge for seven weeks from its opening in December 1826, and during that time 136 patients in fever were treated in these wards. Since the 1st February last, I have had charge of the two men's clinical wards in the Royal Infirmary, the smaller of

which has been kept as a fever ward, and 110 cases of fever have been treated there.

In all, therefore, since the 1st February 1826, 342 patients in fever have been under my care, in circumstances demanding daily reports of their symptoms to be made and preserved.

This number, however, forms but a small part of the patients who have been accommodated and treated in the two hospitals during the last eighteen months. I am happy to say that I believe there has not been, during the whole of that time, a fever patient refused admittance into the hospitals, or one whose admission has been delayed more than twenty-four hours after his application. Indeed, they have in general been admitted within two hours after their cases were certified by any medical man as fever.

The first of the following tables exhibits the whole numbers admitted into the Royal Infirmary in fever in each month from the 1st November 1826 till the 1st August 1827, with the mortality among the admissions of each month. The second exhibits the numbers admitted into Queensberry House in each month that it was kept open, with the sexes of the patients, and the dismissals and deaths that occurred in each month. It does not, therefore, exactly correspond to the former. In both, as will necessarily happen in the business of so large establishments, there must be some slight inaccuracies. A few patients were admitted into Queensberry House who were afterwards found not to have fever; and a few who were not entered as fever patients on their admission into the Royal Infirmary were afterwards found to have fever, and transferred to the fever wards. These errors may nearly compensate one another. It should be added, that it has always been the practice in the Royal Infirmary to strike off the books such patients as die either from fever or other causes, within the first twenty-four hours from their admission; which will of course diminish somewhat both the numbers and the mortality as recorded at that hospital. But this rule has not been followed at Queensberry House, nor in the more particular account which I propose to give of the mortality in the clinical wards under my own care.

TABLE I.

Fever patients admitted into the Royal Infirmary between the 1st November 1826 and 31st July 1827, inclusive.

Nov. 146, of whom 10 died.	April 94, of whom 7 died.
Dec. 131, of whom 17 died.	May 56, of whom 5 died.
Jan. 67, of whom 5 died.	June 75, of whom 7 died.
Feb. 77, of whom 10 died.	July 138,* of whom 7 died.
Mar. 109, of whom 8 died.	Total 893, of whom 76 died.

Average Mortality 1 in 11 $\frac{5}{7}$

* Many of these patients still remain in hospital; and, of course, some of the cases may yet prove fatal.

TABLE II.

Table of Patients admitted into Queensberry House between
18th December 1826 and 31st June 1827.

Admissions.			Deaths.			Admissions.			Deaths.		
Dec.	{	Males,	30	1	April.	{	Males,	41	6		
		Females,	40	0			Females,	43	5		
		Total,	70	1			Total,	84	11		
Jan.	{	Males,	53	7	May	{	Males,	32	3		
		Females,	91	3			Females,	40	5		
		Total,	144	10			Total,	72	8		
Feb.	{	Males,	56	8	June	{	Males,	8	0		
		Females,	109	15			Females,	7	4		
		Total,	165	23			Total,	15	4		
Mar.	{	Males,	60	9	Total.	{	Males,	280	34		
		Females,	67	11			Females,	397	43		
		Total,	127	20			Total,	677	77		
				Average Mortality,	Males,	1 in 8.118.		Females,	1 in 9.233.		
					Total,	1 in 8.8.					

The whole number of patients in fever, therefore, treated in the two hospitals during nine months appears to have been 1570, of which number 153 have died, or 1 in 10 $\frac{1}{3}$.

The chief cause of the unusually great and rapid extension of fever during last winter was no doubt the very distressed condition of a great part of the lower order of inhabitants, in consequence of the diminished expenditure of the higher ranks, and particularly of the failure of many speculations in building, which had given employment to great numbers of masons, joiners, plasterers, and labourers. Of the extent of this distress some idea may be formed from the fact, that when in January last a subscription was raised to give work at low wages to men who had been thus thrown out of employment,—although none were admitted who could not show that they had been several weeks out of work, and although much pains were taken to verify the statements of applicants before giving them relief,—in a very short time the number of persons employed by the committee for managing the subscription amounted to 1700, many of whom had families, and who were thankful to get work at 5s. a-week. A very great number of the patients received into the hospitals in fever belonged to families of which the working members had been out of employment for periods varying from six weeks to six months; and Edinburgh has furnished but too many opportunities, both recently and formerly, for observing, that it is among such distressed families that fever spreads most rapidly and extensively.

There has been, however, abundant evidence, that, by whatever circumstances the propagation of the disease may be favoured or increased, the mode of its propagation is for the most part *by contagion*; and it is worth while to state the evidence which I apprehend to be conclusive on this point, because it appears to me that in some recent discussions on the subject of contagion, the grounds on which that property is generally ascribed by the medical men of this country to continued fever have been misrepresented.

It is quite obvious, as has been stated by Dr Armstrong and many other authors, that the mere circumstance of many persons being simultaneously or successively affected with fever in the same house, or in a limited district, is no proof of the disease spreading by contagion, because the fact may be explained by the supposition of a local cause, such as a malaria, acting on them all. But I do not think that the general conviction of the contagious nature of fever which prevails in Britain has been derived from the observation of this fact only. The opinion is rested, I believe, very generally, not merely on the simple observation of the rapid succession of cases within limited districts, but on *comparative observations*, which show that it is the *circumstance of close intercourse with those already sick*, and not any local peculiarity, that is attended with this result. Many such have presented themselves to me during twelve years of attendance at the New Town Dispensary, (during which time fever has twice prevailed epidemically for many months throughout the town, and often spread through single houses or small districts,) and have appeared to me to have all the weight of the *experimentum crucis* as to the question of malaria or contagion. These observations may be classed under the following heads. The phenomenon to be explained is the succession of cases of fever in houses or limited districts; and the facts are stated, not as instances of the succession, but as proofs of its dependence on the circumstance of intercourse with the sick.

I. The very same houses and districts in which a succession of cases of fever is observed in one season, (chiefly in persons holding close intercourse with those already sick), will remain, during a succession of other seasons, perfectly free from the disease, although inhabited by persons liable to it, although equally crowded, dirty, and ill ventilated, and although the disease be prevalent in other, even neighbouring parts of the town. This continual change of the seat of epidemic fever has always appeared to me nearly incompatible with the idea of its depending on a malaria. I have seen such successions of fever cases at one time or another, certainly in hundreds of places in the

town, and I can say positively, not only that there is no one of these in which it prevails every year, but that there is no one from which it has not been absent for a succession of years within my own observation, even when prevailing in the vicinity.

The district in which, during the former epidemic, between 1817 and 1820, the greatest number of fever cases occurred to my observation was a small part of the Cowgate and adjoining closes, 140 yards by 100, in which about 200 cases occurred in less than a twelvemonth.* From many inquiries made there, I am certain that that district remained quite free from fever for several years together after that time; and even during the present epidemic I have not been able to hear of more than one house in that district in which fever has appeared, although I know two tenements just on its border in which it has been very prevalent, and caused several deaths.

The hospitals in which fever patients are received in this town furnish striking examples of the same general fact. When Queensberry House was formerly occupied by fever patients, every resident clerk and every nurse in the house was successively affected with the disease; and since it was re-opened in December last, the resident physician, two of the clerks, (who have not been resident, but have been several hours a-day in the house), the apothecary, several servants, and all the nurses except two, in all above forty individuals, who had necessarily close intercourse with the sick there, have had fever. If this be the effect of a malaria, it must be a very virulent and effective one, and it is reasonable to expect that some record of similar visitations in the former history of the building would be found. But Queensberry House has existed for about a century; it was long occupied as a private dwelling-house by the noble family of that name, afterwards it was occupied by a number of families, and afterwards as a soldiers' barrack; and yet no record can be found of its having been, during these changes, the seat of an epidemic fever. If a malaria has existed, therefore, in that house, it must, on both occasions, have sprung up exclusively at the times when fever patients were removed thither, and lasted only during their stay.

During the present epidemic, as well as in that of 1817-19, many of the clerks and nurses employed in the Royal Infirmary have taken fever. Since November last, six of the clerks employed in the clinical wards only, four of those employed in the ordinary wards, and twenty-five nurses or servants have taken fever. All these persons had necessarily frequent and close intercourse with the fever patients in the house, hav-

* Report of the New Town Dispensary in this Journal, xiv. 123.

ing been employed more or less constantly in the fever wards, excepting only four of the servants. Of these four, two had been employed in the laundry where the linen from the fever wards was washed; one was a porter employed at the gate, who would of course have communication with the fever patients at their entrance and dismissal, as well as with their relations coming to visit them; and one was a nurse employed in the servants' ward, but who was in the habit of visiting the fever wards.

Now, it is not only notorious that many years have often elapsed without any clerk, servant, or nurse living in the Infirmary, or any of the students attending it, being seized with fever, but it is also certain that in this very season those of its inhabitants who have not had intercourse with fever patients have almost uniformly escaped the disease. Of the inhabitants of the ground floor of the house, (including the patients in the lock ward,) none but those already mentioned as having washed the linen from the fever wards, and the barber who shaved the heads of the fever patients, have taken the disease; yet in the case of a malaria it is the ground floor of a house that is generally found the most dangerous. No one of the nurses, whose duty has confined them to the medical or surgical wards, where no fever patients were admitted, has taken fever, with the single exception of the woman in the servants' ward above-mentioned; and of the numerous patients in these ordinary wards, the only one who has taken fever, within my knowledge, during the present year, was a patient in the mens' general clinical ward, who lay in the bed next the door that communicates with the clinical fever ward. If there be a malaria in this house, therefore, it would seem to restrict itself in point of space, as at Queensberry House in point of time, to the immediate vicinity of the fever patients.

II. While it appears that there is no part of the town subject to regular or even frequent visitations of fever, experience shows equally clearly that there is no part of it in which, when fever once occurs, a rapid succession of cases may not be observed, provided that frequent and close intercourse be kept up between the healthy and the sick. I have seen this myself under such circumstances in every possible variety of situation in Edinburgh and its vicinity, high and low, damp and dry, or even airy and confined, and often in situations widely different from those in which the malaria that produces intermittent fever is known to be chiefly generated. For example, when a person has fallen ill of fever, and lain some time in the upper story of one of the common stairs

in the High Street, or adjacent closes, I have often seen a succession of other cases on the same flat, sometimes extending to the next below, while the inhabitants of the lower parts of the same tenement, (sometimes of five or six stories,) have continued free from the disease.

In short, I have seen enough to convince me, that while there is no part of the town in which fever prevails regularly, or very frequently, there is none in which it may not at times spread epidemically.

III. The succession of fever cases in limited districts takes place in some seasons much more rapidly and extensively than in others. But it is certainly a general fact, that it takes place, *cæteris paribus*, always more rapidly and extensively as the intercourse of the healthy with the sick is more close and intimate. Very close intercourse, even in well-aired rooms, is often followed by the effects in question, as appears from the number of nurses, and even of clerks, affected in the hospitals. Frequent and long-continued intercourse in close confined rooms is almost uniformly followed by this result—not in all those who hold such intercourse, but in a large proportion of them, and especially in young persons; and the more airy the rooms of the sick, and the less frequent the intercourse, the fewer instances of successive affection are observed. In the new town of Edinburgh, where almost all the houses are spacious and well-aired, isolated cases of fever are common, and when a succession of cases is observed it rarely extends beyond a few individuals. But in the crowded and ill-aired parts of the old town I can hardly recollect an instance, even during the years when the disease was least generally prevalent, of a patient in fever having lain at home during the whole disease without some other cases speedily following; and in many instances, when the disease has been more prevalent, the succession has extended to ten, twenty, or thirty, within a few yards of the residence of the first patient. All this corresponds exactly to the supposition of the disease being propagated by effluvia arising from the sick; but we know that the malaria arising from the earth, which engenders ague, is *much less* virulent in crowded than in thinly inhabited districts; and although it is certainly possible that there may be another kind of malaria generated within houses, or even in single rooms, and which may be increased by imperfect ventilation, yet it is hardly possible to conceive how the circumstances of crowding, or closeness of intercourse with the sick, should increase the efficacy of such a malaria, as they certainly do increase the tendency of continued fever to spread.

IV. The efficacy of intercourse with the sick in producing the successions of fever cases has seemed to me to be strikingly exemplified in several instances, in which fever has clearly appeared to be imported into particular districts, by persons whom I knew to have either had the disease themselves, or to have had close intercourse with the sick in other places. Two examples of this kind are I think worthy of being put on record.

Some years ago, at a time when there was no great number of fever cases in Edinburgh, I met with a case in the son of a shoemaker, who was lying in a room in which his father and two apprentices were at work. I could not prevail on the father to remove his son to the hospital, although I stated the danger of the apprentices being affected. Within two or three weeks after, I found that the two apprentices were lying ill of fever in their own houses, one of them 200 yards, the other half a mile distant from the workshop, and widely distant from each other. These young men likewise lay at home during the fever, and each of their cases was speedily followed by a succession of others in the inhabitants of the rooms which they occupied, and of those immediately adjoining, who had never been at the workshop. In one of these houses seven, and in the other twelve, were thus affected. Now, on the supposition of the fever being contagious, all this was to be expected, and all corresponded to the predictions which were hazarded on that belief. But, on the supposition of such succession of fever cases depending on miasmata, there must have been at least two, more probably three separate and accidentally concurring miasmata to explain the phenomena here observed, —one at the workshop, and one at each of the houses of the apprentices; and there must have been this extraordinary coincidence, that at each of these last the malaria sprung up just at the time when a patient was lying there ill of fever, which he had apparently contracted elsewhere. Farther, the three houses in which these successions of fever cases were observed are in situations very different from one another; and all of them have been, to my knowledge, perfectly free from fever for years together both before and since that time, notwithstanding that fever has been much more generally prevalent, and that they have been inhabited by successive families. What probability is there that three separate miasmata should have arisen in these three houses just at the time when their presence was required in each, to produce an effect which had been foretold as the consequence of another cause undeniably operating on all?

In the beginning of last winter, a family, consisting of a la-

bourer, his wife, and four children, long out of employment, became affected with fever; the mother first. During her convalescence the father and two sons were sent to the Infirmary, and she and the others were turned out of their house when still very feeble, and found shelter with an acquaintance. When the father and sons left the hospital the whole family again removed to a third house, considerably distant from the former, taking with them only some miserable dirty clothing. After many inquiries I could not find that there had been previously any fever in any of the three places which they thus successively inhabited; but it is certain that many inhabitants of the same floor in which they first lived, (and no others in that neighbourhood,) had fever immediately after them;—that in the little court to which they next removed, thirty cases of fever occurred within a few weeks after they went thither, the inhabitants of the same room with them being the first affected;—and that from the third lodging-house which they occupied in the course of the winter, and within a fortnight after their arrival in it, four patients in fever were received into Queensberry House. Here again we must, on the hypothesis of a malaria, suppose three separate miasmata, and a wonderful accidental coincidence between the springing up of each and the application of the very cause which the other hypothesis assigns as the true explanation of the whole phenomena in question.

Lastly, the dependence of the successions of fever cases on the circumstance of intercourse with the sick has appeared to me in many cases to be clearly indicated by the success of measures (especially the early removal of the sick,) taken to prevent the propagation of the disease, on the supposition of its proceeding from that cause, and which could not have a beneficial effect under any other supposition. This is a principle not susceptible of actual demonstration, but the probability of which increases so much on every repetition of the observation on which it is founded, as to amount ultimately to moral certainty. It is quite clear, that, if the successions of cases in limited districts depended on a malaria in each, these successions should be equally apt to take place when the first patients were removed, as when they remained at home during their illness. But while it is, as I have already said, exceedingly rare to see a case of fever run its course in any of the crowded districts, without other cases speedily following in the immediate vicinity, nothing is more common than to see single cases not followed by any others, when they are removed to the hospital within a few days from their commencement. What was stated as the result of my experience in the New Town Dispensary during the former

epidemic is strictly applicable to all that I have seen in the present. "We should have little difficulty in pointing out above a hundred houses where a single case of fever has occurred, where the patient has been speedily removed and the place cleaned, and where there has been no recurrence. But we should hardly find five houses in all the closes of the old town in which a patient in fever has lain during the whole, or even during half of his disease, and in which other cases of the disease have not speedily shown themselves."*

I have stated the results of my own observations on the cause of the propagation of the disease, without any reference to the statements of others; but it were easy to show, indeed it must have occurred to most readers, that they correspond exactly with what has been observed by those authors, whose writings on the subject of contagion are most esteemed in this country. Taking all the facts above stated together, and comparing them with the concurring testimony of many other observers, I must profess myself to be not only satisfied with the evidence on this point, but also at a loss to understand what better evidence can possibly be expected. At all events, it is plain that the evidence rests entirely on *comparative observations*, and is very different from that which is furnished merely by the *simple observation* of a number of fever cases occurring simultaneously or successively in a limited district; and that it cannot be refuted merely by pointing out the fallacy attending conclusions drawn from such simple observations only.

No doubt it is difficult to understand why, of a given number who have intercourse with the sick, some only should be affected, and others should escape; but the same difficulty attaches itself to almost every other instance in which a disease is referred to its exciting cause, there being no cause of disease which acts with perfect certainty and uniformity. And at all events, the same difficulty attaches itself to the explanation of the diffusion of fever by the supposition of a malaria. To start this difficulty is in reality to express a doubt, whether the fact of the successive affection of persons living near one another is one so generally observed as to demand an explanation at all; whereas it is allowed, I believe by all inquirers, that this fact, though not a constant and invariable one, is yet one so generally observed as to demand some explanation; and the only question is, what explanation is the most consistent with the phenomena?

It should not be overlooked, in any discussion regarding the contagious nature of fever, that in those seasons when it pre-

* See Vol. xv of this Work, p. 312.

vails most epidemically, and very remarkably in the present, it is very frequently attended by an eruption of the skin, closely resembling what is seen in some of the contagious exanthemata, particularly the measles. A majority of the cases treated by me in the hospitals during the present year had more or less of this eruption. It was most distinct and numerous in severe typhoid cases, but not confined to cases of any peculiar type. It was very irregular in its time of appearance and disappearance,—seldom appearing, however, before the fourth or fifth day, sometimes not till the tenth or twelfth, in which last case it was attended, I think uniformly, by an aggravation of the febrile symptoms, and was of course a very unfavourable sign. It very seldom assumed the appearance of true petechiæ. Such cases of spotted fever may be said to form the link that connects the order of fevers with that of contagious exanthemata.

In speaking of the Pathology and Treatment of fever, it is impossible to leave theoretical speculations entirely out of view ; but I shall endeavour to confine my remarks to such speculations as have the most direct bearing on practice.

It is quite obvious, independently of all theory, that in different cases of fever death takes place in very different circumstances. Some die comatose, a few even early in the disease, the pulse being firm almost to the last,—certainly after all hope of recovery is over : These exhibit as unequivocal examples of death *beginning at the head*, as fatal cases of apoplexy or hydrocephalus. Others die manifestly asphyxiated, the respiration being first much impaired, and the pulse sinking only after the urgent dyspnœa and lividity of countenance have indicated that the blood is not sufficiently arterialized for the support of life. I have seen several fatal cases of fever in which death appeared as decidedly *to begin at the lungs* as in pneumonia or hydrothorax. Others again, without loss of sensibility, and without any material affection of the breathing, either sink rapidly soon after the symptoms of fever have appeared, or are more gradually exhausted by the diseased actions ; and in them death takes place rather in the way of simple *asthenia*, than of coma or asphyxia. This occurs in some cases without any local affection of any importance being observed ; while in others it seems manifestly to depend on complication of the fever with diseased action in the abdomen, denoted by vomiting, pain, tenderness, diarrhœa, or melæna. Farther, it is very often obvious, in the progress of the same case, that there is danger of a fatal termination, both in one or other of the two first of these modes, and afterwards in the last.

There can be no doubt, therefore, that the danger of fever,

so far as can be judged from the symptoms, depends very often on complication of the symptoms which we regard as characteristic of fever, (because they are never absent in the cases to which we give the name), with others indicating various local affections which we have not the same grounds for considering to be essential to its nature. On this account it appears to me, that in a regular course of instruction on pathology or practice, the subject of fever may be most properly treated *after* the history of the different inflammatory diseases of internal parts has been given.

It is only by these considerations that we can account for the great difference of opinion which has subsisted and still subsists regarding the value of different remedies in fever; and they at the same time instruct us, that, with a view to the due appreciation and proper selection of these different remedies in individual cases, the most important study is that of the *various modes of fatal termination* which may be observed in this disease; or that, as it was long ago expressed by Dr Cullen, "the indications of cure must be formed upon the view of obviating the tendency to death." This tendency depends evidently on various causes, and shows itself in various ways, in different epidemics, different climates and seasons, different individual cases, and even in different periods of the same case; and it is only in so far as any remedies used in fever appear fitted to counteract that particular tendency which is most to be apprehended in that individual case, and at that period of the disease, that its use can be said to be rational or scientific.

With these views, I am induced to hope that a short review of the chief varieties and complications of fever that have been observed in a large number of fever patients; including, for a reason that will be presently mentioned, more than an average proportion of severe and fatal cases, and whose symptoms were carefully recorded, may be considered of some value.

It must be observed, however, that no numerical statement of this kind can give all the information which it is desirable to convey regarding the circumstances of the fatal cases. In almost all these, more or less complication of the general symptoms of fever with local affections not essential to the fever is observed; but it seldom happens that the degree of this local affection is such that it can be fairly regarded as the sole cause of death. The more dangerous affections of the head, for example, so often seen in the fevers of this country, the urgent headach at first, and then the more active delirium, tremors, spasms, coma, all the symptoms of the "*fièvre ataxique*," appear manifestly to be in general connected with increased vascular action, and a condition approaching, if not amounting to inflammation within

the cranium. They nearly resemble the symptoms of decided inflammation in these parts. They are often attended by increased flow of blood to the face and eyes. When fatal, they very often leave behind them more or less of serous effusion on the different surfaces of the membranes within the head. In a few cases they are followed by palsy, as I have myself repeatedly known; and in many cases they are decidedly relieved by the use of remedies which manifestly diminish the force of vascular action, particularly in the head, or cause derivation to other parts,—namely by bleeding, leeching, blistering, purgatives, nauseating medicines, and warm fomentations of the lower limbs.

But, on the other hand, we have equally good evidence that neither these symptoms themselves, nor the danger of the cases in which they occur, can be ascribed solely to an increased or inflammatory action within the head. When such cases are fatal, although most generally more than usual of serous effusion is found in the head, yet sometimes nothing is found there which can be pronounced a morbid appearance.* When effusion is found, its quantity bears no fixed proportion to the intensity of the nervous symptoms preceding death; and never (so far as I have observed,) amounts nearly to the quantity that is usually found in the true hydrocephalus, which is now generally, and I have no doubt justly, regarded as an inflammatory disease. Although many patients who have these symptoms in fever die, yet others who have them in an equal or even greater degree of intensity recover; and this difference very often appears to depend on the circumstance of the vital powers moving the blood being less depressed; the pulse being less frequent, and firmer in the latter cases than in the former. Farther, although these symptoms in fever may often be moderated, they cannot with certainty be prevented, and can very seldom be altogether removed by the use of antiphlogistic remedies, their ultimate and complete disappearance being in general apparently the work of nature. Many of them are often seen to diminish and disappear in cases manifestly of extreme danger, not only without the use of any such remedies as can subdue active inflammation, but even under the use of stimulants, (when these appear to be demanded by the weakened state of the pulse,) and of other remedies which have no power over inflammation; and when they disappear under the use of these means, they very seldom leave any such consequences be-

* “ Pour nous il est bien démontré, qu’il n’est guère de symptôme nerveux qui ne puisse se manifester sans lésion appréciable du cerveau et de ses dépendances.” Andral, *Clinique Médicale*, t. i. p. 418.

hind them as would indicate that the substance of the brain has undergone any disorganization.

Similar observations apply to the cases in which fever is complicated with dangerous local affections in the thorax or abdomen; and they appear to me sufficient to show, first, that an increased vascular action, approaching to inflammation in different parts, occurs in the greater number at least of these more complicated cases of fever, and contributes to make them dangerous or fatal; and secondly, that the danger or fatality of these cases does not depend simply on this increased local action of vessels, or its consequences, but partly also on the depressing or debilitating influence of the disease on the circulating system;—an influence which can for the most part be distinctly observed, and is sometimes even fatal in cases where no local disorder bearing any of the characters of inflammation can be detected either during life or after death. The great difficulty, therefore, in the management of complicated cases of fever consists in rightly balancing between the amount of danger from local affections, not essential to the febrile state, and that from the general depression of the sanguiferous system.

These are the general principles, regarding the pathology of dangerous cases of fever, which it appears to me of the most importance to inculcate; and the present epidemic has furnished but too many examples which appeared to me to illustrate them all. I have no difficulty in saying, that in this epidemic, and particularly during the present year, the danger has appeared much more frequently to depend simply on *asthenia*,—on the weakened state of the circulation, than during the last epidemic between 1817 and 1820. Most of the fatal cases were indeed as usual, evidently complicated with local affections, which no doubt contributed to the fatal event: but in the greater number of such cases the local affections appeared both from the symptoms before death, and from the appearances after death, to have been such as might probably have been got safely over if the general strength had not failed.

This opinion must, of course, rest chiefly on the observation of individual cases, and does not admit of direct proof. The difference of epidemics, in this respect, is a difference in degree, not in kind, and cannot be stated in numbers, or reduced to calculation. But one general fact may be adduced in support of the opinion above stated, viz. the unusually great mortality in old persons, and those of broken constitutions, during the present epidemic.

The whole mortality in the hospitals during the present year appears, from the table already given, to have been greater than during the last epidemic; and a great share of the mor-

tality has, as usual, fallen on the clinical wards, many of the clinical patients having been selected from among the applicants on account of the apparent urgency of their symptoms. In the clinical wards, while under my care during 1826, the deaths from fever at different periods of life were as follows:—

	Cases.	Deaths.	Proportions.
Under 15, -	21	2	1 in $10\frac{1}{2}$
From 15 to 30,	49	2	1 in $24\frac{1}{2}$
— 30 to 50,	24	7*	1 in $3\frac{1}{2}$
Above 50, -	2	1	1 in 2
Total,	96	12	1 in 8

Of the eight deaths beyond the age of 30, six were in persons manifestly of feeble habit, and three of these at least were notoriously addicted to the use of spirits, which is the circumstance that appears most of all to aggravate fever among the lower orders. The head symptoms in these cases are unusually urgent, and the pulse at the same time generally feeble, and apt to sink after evacuations.

In the present year the mortality from fever in the men's clinical ward has been still greater; and the ages of the patients as follows:—

	Cases.	Deaths.	Proportions.
Under 15, -	25	0	—
From 15 to 30,	48	6	1 in 8
— 30 to 50,	27	5	1 in $5\frac{1}{2}$
Above 50, -	10	5	1 in 2
Total,	110†	16	1 in $6\frac{7}{8}$

It should be mentioned, that of the sixteen fatal cases three died within twenty-four hours from their admission, and in a fourth, who died forty-eight hours after admission, it was doubtful whether the disease, (of which the chief symptoms were jaundice, slow pulse, and delirium, soon followed by coma, and dilated pupils,) was really fever. On the other hand, we have had no less than five deaths in the present year from the sequelæ of fever after the true febrile symptoms had declined. Of these one was from phthisis, known to exist, and far ad-

* I do not include here one man, aged 33, who, when his fever, which had been very severe, was declining, was seized with erysipelas, which carried him off in two days.

† I do not include here several cases of young persons who had been living along with fever patients, and came in with the usual symptoms of incipient fever, but got well so soon after bleeding, vomiting, and purging, that the nature of their complaints may be considered doubtful.

vanced before the patient took fever, one from chronic pleurisy, with inflammation of several joints, and sloughing ulcers of the back and legs, succeeding fever, which had itself succeeded small-pox; and three from erysipelas, which has been most unfortunately prevalent this summer in the ward occupied by these men in fever. Of the ten beyond the age of thirty who died in the course of fever, four were men of evidently broken constitution, and four more, though in appearance of tolerable strength, were known to have been long drunkards.

Of the patients who were under my care in Queensberry House last winter, (and who, of course, were admitted without selection,) the mortality at the different ages was as follows:

	Cases.	Deaths.	Proportions.
Under 15, -	37	0	
From 15 to 30, -	52	3	1 in 17
— 30 to 50, -	42	5	1 in 8½
Above 50, -	5	1	1 in 5
Total,	136	9	1 in 15

In all, of 342 patients there have been

	Cases.	Deaths.	Proportions.
Under 15, -	83	2	1 in 41½
15 to 30, -	149	11	1 in 13½
30 to 50, -	93	17	1 in 5½
Above 50, -	17	7	1 in 2½
Total,	342	37	1 in 9½

The great increase of mortality in advanced life would have been more apparent, but for several fatal cases of affection of the chest in fever, which occurred in rapid succession in the clinical ward in February 1827. As it stands, however, the difference is decidedly greater than was observed in the last epidemic. We have an account of the mortality at Queensberry House in 872 cases during the year 1819 in Dr Welsh's book on the Use of Blood-letting in Fever, and at that time it was, below the age of 20, one in sixty-five, and above the age of 50, one in eight.* In 678 cases recorded by Dr Bateman,

* The whole mortality in that year at Queensberry House was not more than one in twenty-two, not one-half of what it has been lately. It is to be observed, however, that the observations of Dr Welsh were made during the decline of an epidemic which had begun in 1817, whereas the present epidemic in Edinburgh had hardly begun before last winter. The greatest mortality is known to be generally in the earlier stages of an epidemic. The difference of the mortality in Queensberry House in 1819 and 1827, the hospital having been in both years for the most part under the care of the same physicians, (Drs Home and Duncan,) may be taken as a clear indication of the greater malignity of the disease in the present season, [Author.] In 1817, the mortality was 1 in 14½, in 1818, 1 in 19½. [Editors.]

which occurred in the London House of Recovery in 1817-18, the mortality under the age of 17 was one in twenty-nine, and above the age of 50 it was not quite one in four.

The general fact of the much greater mortality of fever in advanced than in early life seems to me to be of greater importance than has generally been represented, as clearly showing that the danger of the disease cannot depend simply on violence of febrile reaction, and therefore as limiting our confidence in remedies which act merely by opposing that reaction.

Another important particular in which the present epidemic appears hitherto to have differed from the last is, in being more fatal among the lower orders than the higher; the reverse of which is well known to have been lately the case both in this country and Ireland. But during the present year, so far as I can learn, there have been few fatal cases of fever among the higher ranks. I have myself seen, during the time to which these remarks apply, fifteen cases of fever among the gentlemen attending the university, all of which have terminated favourably; and although there have been many other cases among these gentlemen I have heard only of one death.

It may be thought paradoxical, and even presumptuous in me to call in question the beneficial effects of ventilation and pure cool air on the symptoms of fever, on which so much stress has been laid by many authors. But having had occasion to see at least as many patients in fever in the houses of the lower orders in the old town of Edinburgh, (in which ventilation and pure air are very generally quite unattainable,) as I have seen or treated in hospitals, I cannot avoid saying that my experience does by no means confirm that observation. That persons habitually living in spacious and well-ventilated rooms would have fever in an aggravated form if confined in close and dirty rooms, is very probable; but those accustomed to inhabit the miserable, ill-aired, and dirty parts of the town appear to me to pass through fever in their own houses, with some differences indeed in the symptoms, but in general at least as favourably as those that are removed to the hospitals. The difference in the symptoms, so far as I have observed, is just of the kind described by Dr Bateman, (*Succinct Account of Contagious Fever*, p. 136,) although I think he has somewhat overrated its degree, viz. that pure cool air acts as a stimulus, and produces a depressing excitement in those previously confined to close rooms. There is probably less typhoid tendency in the earlier stages, and more demand for evacuations in the hospitals than in the houses of the poor; but the duration of the disease has generally appeared to me to be longer, and the complications of local affections in the later stages to be more frequent and dangerous. This

has been particularly the case when the patients have lain some time, and been already much weakened by the disease, before their removal. I have seen, especially in persons advanced in life, so many cases, previously mild, take an unfavourable turn on being removed to the hospital in the second week of the disease, that unless there be some circumstances peculiarly demanding it, I have ceased to recommend such removal later than the eighth day of the disease; more particularly, as when a patient has lain so long at home, he may generally be supposed to have already communicated the disease to his family. In these circumstances my general advice is, to let the person already ill, particularly if he is old or weakly, lie where he is, and to remove immediately the next that falls ill. There are no doubt cases of fever that require more cordials, more careful nursing, dressing of sores, &c. than can be easily attained in the houses of the poor; but, so far as I can judge from my own observation, fever hospitals are of infinitely greater importance for preventing the diffusion of the disease among the healthy, than for preserving the lives of the sick.

Conformably to the principles above stated, and to the practice of several recent authors, Drs Bateman, Percival, and others, I have endeavoured, in lecturing on the cases of the fever patients, to arrange them according as they exhibited the symptoms of fever in its simpler, or in some one of its more complicated forms; and have afterwards arranged these last according as the local affections which appeared of chief importance were in the head, chest, or abdomen. The arrangement of fever cases in this way must necessarily be somewhat arbitrary.

Dr Bateman, in his valuable and faithful description of the character and varieties of the London fever of 1817-18, has described, as one of "the more complicated forms of typhus," a set of cases of frequent occurrence, but which I have been accustomed to arrange as simple fevers, because, although there are "confusion, wandering, and slighter stupor," indicating considerable affection of the sensorium, yet the danger has appeared, as he states, to be "from mere exhaustion of the vital powers, without any supervention of more malignant symptoms, without stupor or subsultus,"* *i. e.* the danger has appeared to be of death in the way of simple *asthenia*, not in the way of coma.

The line of distinction which I have attempted to draw is this,—that where there has been no threatening of a fatal ter-

* See Succinct Account, &c. p. 52. *et seq.*

mination otherwise than by simple asthenia, and no symptoms of local affection which appeared to exhaust the patient, I have called the case simple fever; but where there have been symptoms indicating such a degree of abdominal disease, (generally some form of diarrhœa,) as seemed likely to exhaust the patient, I have described the case as fever with affection of the abdomen. In like manner, where there has been either so much insensibility, (with or without spasms,) as to threaten death by coma, or such violent and continued pain of head, or such active delirium, as are known generally to subside into coma, unless quickly subdued, I have called the case one of fever with affection of the head;—and again, when there has been so much hurried or laborious breathing, with or without cough or pain of chest, as seemed to threaten death by asphyxia, and render the prevention of that mode of fatal termination the chief object of practice, I have called the case fever with affection of the chest.

Applying the terms in the way now stated, I have arranged the cases of fever treated in the four clinical courses, and in Queensberry House, under my own care, as follows:

Simple Fever.		Fever with Affection of the					
		Head.		Chest.		Abdomen.	
Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.	Cases.	Deaths.
17	1	Infirmary, 11 3		Spring 1826. 11 1		5	0
17	0	Infirmary, 15 5		Summer 1826. 11 3		9	0
25	0	Infirmary, 17 5		Spring 1827. 9 3		5	1
23	1	Infirmary, 16 5		Summer 1827. 10 1		5	0
88	1	Queensberry H. 29 3		Winter 1826-7. 17 5		11	0
170	3	Total. 79 21		58 13		35	1

In all, then, there have been 170 cases which I considered as Simple fever, and among these only three deaths. All the three had delirium, and two of them had a certain degree of stupor; they differed only in degree from the next class of cases to be noticed, the degree of insensibility simply not being such as would have appeared dangerous, but for the sinking of the circulation, which was manifest long before death. In the only one of them, in which dissection was permitted, no appearance was found that could be decidedly pronounced to be morbid. It

may be observed, that under this head of simple fever a number of cases are included, in which there was apparently a threatening of dangerous complications at first, and which may probably have been reduced to the form of simple fever by the evacuations practised.

There have been in all seventy-nine cases in which the Affections of the Nervous system predominated and evidently constituted a great part of the danger, * and among these seventy-nine there were twenty-one deaths. But I have already stated, that even where the affections of the nervous system existed in a degree that seemed quite sufficient of itself to indicate danger, it very often happened that they were combined with such feebleness of pulse, and other marks of failure of the circulation, that death might be said to take place as much in the way of asthenia as of coma. In most of these cases the comatose tendency with spasm and subsultus, and the failure of the circulation come on simultaneously and gradually; but in some there was a state of restless active delirium, with animation of countenance, liveliness of the eyes, and contraction of the pupils, after the voice had become faint, the extremities cold, and the pulse very feeble, or almost imperceptible, constituting the condition which is described by Mr Travers as "Prostration with excitement," and which suggested the remark of Andral, that there is "*dans plusieurs cas de fièvres graves, une excitation factice, aussi bien qu'une fausse prostration.*" This state generally subsided rapidly into absolute and speedily fatal insensibility. Of the twenty-one fatal cases, I considered only five to be examples of death strictly in the way of coma. The duration of the fatal cases under this head was from nine to twenty days, but seldom above fifteen.

We had dissections of fifteen of the twenty-one fatal cases under this head. The only appearance found in any of them which I could regard as morbid was effusion of serum, chiefly beneath the arachnoid, and in the ventricles. In judging whether the quantity of serum found in these situations was to be accounted certainly morbid, I have taken as a guide the observation of Andral, that in order to be so, the effusion should be "*assez considerable pour soulever notablement l'arachnoïde, ou distendre les ventricules;*" but so far from finding, as he has done, such amount of effusion very rare, it has appeared to me the most frequent of all the morbid appearances observable in fever. In twelve at least of these fifteen cases it seemed to me obvious that the arachnoid was partially elevated, and the ventricles distended beyond their usual dimensions. But the quantity of effusion was never great. What was found in

* Cases of the "Fievre Ataxique."

the ventricles never exceeded half an ounce, and did not exceed two drachms in more than three cases. Farther, the quantity of this effusion bore no proportion to the degree of affection of the nervous system before death. In some who had these symptoms violently it was well marked; but in a boy who died decidedly comatose, (his pulse being full almost to the moment of death,) it was very slight, not exceeding a drachm in the ventricles, and being hardly perceptible beneath the arachnoid. In a woman who had, among other nervous symptoms, repeated and general convulsions, it was equally inconsiderable; and in the case already mentioned of a young man who was jaundiced, and died quite comatose, with dilated pupil and stertorous breathing, the quantity found in the ventricles hardly exceeded half a drachm, and there was positively none beneath the arachnoid. Such an amount of effusion as is found in the heads of many fever patients may be regarded, I think, as a sign that an increased action of vessels has taken place there; but by no means as a proof that such increased vascular action has been the cause of all the nervous symptoms.

In the treatment of this form of fever, when seen from the commencement, I have no doubt that blood-letting is almost always a proper preliminary to all other measures; but after the pain of head has subsided, although the pulse be of good strength, its beneficial effects are less decided. The use of cold applications, especially to the head, and fomentations of the legs, and the pediluvium, are generally and justly esteemed. In some cases of active delirium, with pretty firm pulse, nauseating and purging doses of tartar emetic have appeared a very useful auxiliary. When the chief nervous symptoms are the comatose tendency and spasms, blistering the head and neck, and continued though not violent purging, appear to be the most suitable evacuations, and I have seen many striking examples of their efficacy. It may be worth while to state, in regard to purgatives, that they would appear to be useful chiefly on the simple principle of derivation, and the appearance of the stools can by no means be taken as a guide either to their use or discontinuance. I have many times used them for days together with what appeared to me manifest good effect on the nervous symptoms, although nothing in the slightest degree morbid could be detected in the evacuations; and, on the other hand, when the most urgent symptoms have been those of mere debility, I have withheld purgatives, although the stools were very dark-coloured and offensive, and have had the satisfaction of seeing the natural appearance restored, as the general febrile symptoms improved, without the use of any means specially directed to this end. The pulse, notwithstanding the fallacies so often attributed to it, has appeared to me a much surer in-

dication of such strength of the circulating system as demands evacuation, than the heat of the skin. I have seen the heat, in various cases, as high as 103 and 104, when the pulse was soft and feeble, and the disease advanced, and when gentle stimulants appeared to me to be required, and were used with success.

When the nervous symptoms in the advanced stages of fever have been accompanied with manifest weakness of pulse, I have used wine in small but often gradually increasing doses, and camphor or ammonia, along with the evacuations now mentioned, or even, where the depression has been very great, without them; and have thought them much more frequently and decidedly useful in the present epidemic than formerly. In reflecting on some of the fatal cases, I have thought that these remedies had been too soon given, and in others that they had been too long delayed; and I have no doubt, from watching the progress of many such cases, that errors may very easily be committed in both ways, even by persons conversant with the disease. The greatest difficulty has appeared to me to be in judging of the probable effects either of evacuations, or stimulants, or anodynes, in persons habituated to strong liquors, and whose symptoms in fever resemble those of delirium tremens.

Opiates have appeared of most essential service in some cases where there was much slighter delirium, and restlessness, without pain of head or flushing, therefore generally after loss of blood. I have used them repeatedly in such circumstances so early as the fourth or fifth day with this marked good effect, that sleep has followed their use; and after one or two repetitions of the dose, the patient has passed into the state of drowsiness without stupor, which is known to be one of the most favourable forms of simple fever, and has had no farther occasion either for anodynes or other active remedies. The more violent tremors and subsultus of the advanced stage of fever have generally been combined with so much stupor as appeared to render the full use of opiates hazardous, and where they have been tried under such circumstances they have not appeared to me successful.

Of fifty-eight cases in which the Function of Respiration was embarrassed at one period or other of the disease, in a degree which appeared to indicate danger, thirteen were fatal; but of these four only were fatal strictly in the way of asphyxia; in the other nine there was extreme debility, but no such urgent dyspnœa towards the close of life, as appeared from comparison with other cases, in which the pulse continued firmer, to be incompatible with the restoration of health. Indeed, in several of these last the breathing had become so easy many hours or

even days before death, that the amount of disorganization of the lungs found after death excited much surprise. The duration of the fatal cases under this head was from twelve to twenty days. The cases of those who recovered from this complication were generally very protracted, in several instances lasting five or even six weeks before the fever subsided; after which, however, the recovery appeared to be complete.

Very few of the cases under this head showed any symptoms of pleurisy, and none who died during the continuance of the fever had the pleura inflamed. In two cases (a child three years old and a young man) the affection of the breathing, which came on late in the disease, and when the strength was much exhausted, was decidedly croupy; and in both, on dissection, inflammatory thickening and tenacious muco-purulent effusion were found on the mucous membrane of the larynx, though not in such a degree as nearly to close the glottis. In general the affection of the chest appeared to be chiefly bronchitis, and this was sometimes evidently very general; but there was often great difficulty in forming a judgment whether or not the substance of the lungs was undergoing inflammation and disorganization, and how far evacuations ought to be carried in the hope of checking these processes; and this difficulty was by no means wholly removed even by examination after death. Dissection was obtained in two only of the four cases in which I have stated that death took place strictly in the way of asphyxia, and in neither of these was there any part of the lungs found so far condensed as not to crepitate when cut. In both the bronchi were much clogged with frothy mucus. In one this was the only appearance decidedly morbid. This was in a young man of slender make, admitted on the tenth day of the disease, and in a state of great debility; he was twice leeches on the breast, but not bled. In the other the lungs were very generally œdematous, and in several places even the superior portions were of a red colour. This was in a very strong young man, whose pulse was full throughout the disease, and who was largely and repeatedly bled both before and after admission, though not for a week before death.

In four of the cases which were considered as fever with affection of the chest, but in which the impeded state of the breathing did not appear to be the chief cause of death, we found on dissection pretty large portions of both lungs so far condensed as not to crepitate when cut, and smaller portions so heavy as to sink in water. In all these the condensation was chiefly in the posterior portions; and consequently, the patients having all lain on their backs in a state of extreme debility for some days before death, it was difficult to say how much of the condensa-

tion was to be considered as the "*Peripneumonie des agonisants*," that is, was to be ascribed to the congestion of blood in the depending parts of the lungs, during the time preceding death, when the strength of the heart may be supposed insufficient to counteract the effect of gravitation on the blood. But although I have no doubt that part of the appearance was to be ascribed to this cause, and although a degree of the change alluded to may certainly be often seen in the posterior portions of the lungs when there has been no ground for suspicion of disease there, yet as the condensation was in each case somewhat greater in one lung than the other, and as it projected forwards towards the anterior surface of the lungs in some places more than in others, we concluded that the appearance might be regarded as indicating inflammation. I must add, however, that two of these patients (treated in Queensberry House) were seen almost from the beginning of their illness, and were bled repeatedly, and to as great an extent as the state of the general symptoms seemed to permit, and I am confident that no one who saw the progress of the symptoms after the last bleeding in these two cases, would have thought us justified in the farther use of the remedy.

Besides early bleeding, leeching, and blistering, the remedies that have seemed to me most useful in cases of this complication have been small doses of tartar emetic, and of ipecacuanha and calomel, with opiates. Many of them have appeared to me to require an allowance of wine when they became protracted, and have improved under it.

The Abdominal Affection chiefly observed has been diarrhœa, with pain of abdomen; sometimes with tenderness; often with the florid red tongue; sometimes with bloody stools; sometimes, however, pain of abdomen and vomiting have been the abdominal symptoms observed; and this was the case in the only one of these cases which proved fatal,—namely, in an old man, who had a very weak pulse and cool skin almost from the first. He had vomiting of matter like coffee-grounds, and died on the eighth day. The only decidedly morbid appearance found in him on dissection was enlargement and induration of the liver. The cases of diarrhœa, with pain and tenderness, were treated chiefly by small bleedings, general and local—by fomentations and opiates, in such quantity as to moderate without abruptly checking the evacuations,—and by mild laxatives occasionally interposed when the stools were slimy and scanty. Some of these cases were very protracted.

In regard to the tendency to diarrhœa in fever, there appears to be a marked difference (perhaps not sufficiently known) between the inhabitants of France and of this country. It is

stated by Andral, (*Clin. Med.* T. i. p. 12 and 14,) that there are few cases of fever in the hospitals of Paris, in which a spontaneous diarrhœa does not occur, and that the greater number of these patients have from five to ten stools daily. I need not say that the opposite state of the bowels is most general in this country during fever, at least in adults. In children, painful diarrhœa is common; and the greater number of the cases under this head in the hospitals were below the age of 15.

A corresponding difference may be observed in the appearances on dissection in fever in the two countries. Andral, although fully aware of the fallacies from pseudo-morbid appearances, states, as the result of his observations, that in twenty-six out of thirty-eight dissections and fever patients there were decided marks of inflammation and its consequences (generally ulceration) in the mucous membrane, most commonly of the small intestines; and fully as large a proportion of unequivocal morbid affections of that membrane is admitted by Chomel. Now, in all the dissections of the fever patients of whom I have here spoken (twenty-five cases where the abdomen was examined) there was only one in which any ulceration could be detected in the mucous membrane, and even in that one a single small ulcer only could be found. I did not think it necessary, indeed, to examine in these cases the whole tract of the intestinal canal, but the lower end of the ileum was always examined, and different other parts of the mucous membrane in every subject, particularly where the least apparent enlargement of any of the mesenteric glands could be observed. In a few cases the glands of Peyer and Brunner were more distinct than usual, but not more so than I have seen them in persons who did not die of fever, or of any abdominal disease. Partial vascularity of the mucous membrane was common; and in some cases the mucus on its surface, at some of these places, was deeply tinged with blood; but these appearances were always chiefly, and in general strictly, confined to the depending portions of intestines; and there was only one case in which they appeared to me greater than I have repeatedly seen in the bodies of persons who did not die of fever, and especially in those who suffered much from dyspnœa before death.

I have seen the ulcerations of the mucous membrane, of which so much has been lately said, in a considerable number of cases of children who had died of fever in this country, but very seldom in adults; and when I have seen them there has been almost always diarrhœa, and generally tympanitic distension of the abdomen, as described by the French authors.

The much greater frequency of the effusions within the cranium in fatal cases of fever in this country, and the much

greater frequency of diarrhœa and of ulceration of the mucous membrane of the intestines in France, seem to me to go far to account for the great difference in the recommendation of various remedies by practical observers in both countries ; and, in particular, for the much higher value which the practitioners of this country set on the use of purgative medicines.

In concluding this paper, I would observe, that the efficacy of evacuations, particularly of blood-letting in the earlier stages, and sometimes even in the more advanced stages and many cases of the more complicated forms of fever, has been fully illustrated in various publications of late years. I have met with cases both before and during this epidemic, in which it appeared to be decidedly exemplified ; and in some which were fatal, I have regretted that more bleeding had not been tried. But I have been more convinced, during the present epidemic, than I had ever been before, of the efficacy of stimulating remedies during a great part at least of those cases of fever, in which, although an evident and dangerous complication of local affections exists, yet the chief danger appears to depend on the general depressed state of the circulation ; and I trust I shall be pardoned for requesting the attention of those of my readers who have been much impressed with the belief of the essentially inflammatory nature of fever to the subjoined cases, which appear to me good illustrations of what is now stated.

I. Thomas Haskins, aged 27, quarryman, Water of Leith, of robust habit, admitted March 12. Complains of much pain of head, and of great general soreness of the trunk and extremities ; is quite free from delirium, and has no tendency to stupor, but slight deafness ; has a slight cough, not causing any pain of breast ; feels some pain on pressure of the epigastrium ; pulse 100, soft and compressible ; heat 103 ; respiration natural ; tongue dry, brown in the centre, white on the edge ; bowels regular ; much thirst ; anorexia.

Eight days ago he was attacked with rigors, succeeded by severe headach and burning heat of the skin. He has taken senna and salts, but has had no medical attendant ; attributes his illness to exposure to cold ; has not been living in any district affected by fever.

Four years ago he had an attack of phrenitis, since which he has had partial paralysis of the right superior eyelid, with imperfect vision of that eye.

Abradantur statim capilli, et habeat Vini Rubri uncias iv.

12th.—Pulse 100, rather weak ; heat 102½ ; tongue furred ; no stool ; slight pain of abdomen ; tendency to stupor, but no delirium or subsultus.

Lavelur caput subinde aquâ tepidâ cum aceto ; foveatur abdomen ; sumat statim Infusi Sennæ uncias ii, cum Sulphatis Magnesiæ drachmis ii. ; et continuetur vinum.

14th.—Pulse 96, feeble; heat 101° ; tongue a little moist; three natural stools passed in bed; stupor increased, but he is distinct when roused. A blister was applied to the nape of the neck last night, and is not yet removed; much subsultus at times, less however since he has taken the wine, which he relishes.

Habeat Vini uncias viii.; R Carbonatis Ammoniacæ scrupulos ii, Aquæ puræ uncias vii, Tincturæ Cinnamomi compositæ unciam. Misce; sumat unciam sextâ quaque hora.

15th.—Blister has discharged a little; has taken ten ounces of wine, and a bottle of porter; no pain, delirium, flushing of the face, or subsultus; pulse 90, rather firmer; tongue dry, with a lighter fur; three stools; urine and feces passed involuntarily; a little sweating last night.

Continuentur vinum et mistura. A bottle of porter.

16th.—Pulse 80, stronger; tongue dry, furred in the centre; heat 98° ; face a little flushed; three stools passed in bed; is very drowsy, but pretty distinct when roused; no subsultus; much general soreness. Has taken ten ounces of wine, very little porter.

Applicetur vesicatorium vertici; habeat vini uncias vi. Half a bottle of porter.

17th.—Pulse 96, soft; tongue moister; has taken ten ounces of wine and his porter, but very little food; blister has given little discharge; no subsultus; less stupor; much sleep.

Sumat statim haustum ex Olei Ricini uncis vi. Habeat Vini uncias viii.; et continuentur alia.

18th.—Pulse 84, pretty full; tongue moist, with a white fur; one stool passed in bed; no delirium or subsultus; says he does not sleep; takes no food.

Repetatur statim haustus ex Oleo Ricini; continuentur alia; et habeat hora somni haustum e liquoris opii sedativi guttis xx.

19th.—Pulse 78, full; tongue less furred, dry; two stools, the last not passed in bed; hardly any food taken; drowsiness; no delirium or subsultus.

Repetantur Vinum et Oleum Ricini. Tea morning and evening.

20th.—Pulse 64; of good strength; tongue still dry in the centre; some return of appetite; stools no longer involuntary.

Continuetur Vinum. Habeat Jusculi Bovini libram indies.

21st.—Tongue moist, brown; pulse natural; bowels open.

Habeat Vini uncias iij. Continuentur alia. Ordinary diet.

He had afterwards an attack of rheumatism, but was dismissed cured on the 13th April.

II. Alexander Davidson, aged 38, admitted July 25. Has a great deal of stupor, and is with difficulty roused so as to answer questions; says he is free from pain, and complains only of debility and want of appetite. Face flushed; eyes suffused, and pupils contracted; skin warm, dry, and covered with numerous spots of a livid colour; tongue dry, brown; some thirst; breathing laboured, noisy, slightly accelerated: pulse 132, small, but of unequal strength and frequency; no stool to-day; has passed his urine in bed; epigastrium not swollen or uneasy on pressure.

He is reported to have been ill four days ; but is unable to give any history of his complaints. Had a purgative draught on admission, with a little wine.

Applicetur Vesicatorium nuchæ ; Injiciatur enema domesticum ; Habeat Vini uncias vi. R Cumphoræ scrupulos ij, Spiritus ætheris Nitrosi et Mucilaginis Acaciæ, singulorum unciam, Aquæ uncias vi. M. Sumat unciam quarta quaque hora.

26.—Blister rose well ; pulse 120, very small, and weak ; tongue dry, and ill protruded ; several dark liquid stools ; hands cold ; face still slightly flushed ; eyes less suffused ; is still inclined to stupor ; little delirium ; countenance pretty lively. No subsultus.

Continuetur Mistura Camphoræ ; Habeat Vini ℥viij.

27.—Has slept a good deal ; is inclined to stupor ; not delirious ; pulse 114 ; rather firmer ; tongue dry, ill protruded ; one stool passed in bed ; extremities warmer ; little heat on breast.

Sumat statim haustum ex Olei Ricini drachmis vi. ; Continuentur vinum et mistura.

28th.—A blister was applied to the head last night ; several stools passed in bed ; urine also passed in bed ; eruption continues ; is drowsy, without delirium or coma ; slight subsultus ; pulse 114, small ; tongue very dry ; skin cool ; extremities of moderate heat.

Continuentur vinum et mistura ; Habeat haustum ex Oleo Ricini cras mane.

29th.—Bowels continue open ; stools and urine still passed in bed ; sleeps much ; no delirium ; tendency to coma continues ; pulse 96, small ; tongue dry, ill protruded. Two small blisters were applied over the ears, and they have given some discharge. A gangrenous eschar over the sacrum ; hands cold.

Habeat vini uncias viij. Intermittatur mistura. R Carbonatis Ammoniæ scrupulos ii, Aquæ uncias vij, Tincturæ Cinnamomi composita unciam. M. Sumat unciam sexta quaque hora.

30th.—Still very torpid and drowsy ; pulse 104, small ; tongue as yesterday ; a little running from the head, which has been dressed with cerate ; slough on the back as yesterday ; three stools ; face still somewhat flushed ; eyes less suffused.

Sumat statim haustum Olei Ricini. Continuentur alia ut heri.

31st.—Pulse 96 ; tongue somewhat moister ; eruption somewhat faded ; three stools passed in bed, dark-coloured ; more tremor.

Continuentur vinum et mistura.

Aug. 1st.—Sleeps much and quietly ; pulse 100, small ; tongue rather moister on the edge ; skin rather cold.

Continuentur vinum et mistura. A pint of porter.

6th.—He has continued to improve ; pulse 60 ; tongue moist ; takes food ; bowels regular, and stools passed naturally ; sore on the back healing.

III. Samuel Erskine, aged 45, founder. In the ward since 19th February, much emaciated. 12th April.—10th day of relapse of fever. Had delirium and restlessness last night ; had the pediluvium, and has taken five half grain doses of tartar emetic, which caused purging. Had some sleep ; is quiet, but incoherent ; pulse 120, soft ;

tongue dry ; complains of weakness ; no pain ; an indistinct eruption on the breast and arms ; some deafness ; no subsultus.

Habeat vini rubri uncias iv. et horu somni haustum e liquore opii sedativo. R Camphoræ scrupulos ij, Mucilaginis Acaciæ, et Spiritus Ætheris Nitrosi, singulorum nnciam, Aquæ fontanæ uncias xj. Misce. Sumat unciam sextâ quaque hora.

13th.—Was restless and delirious again last night, and pulse was nearly 140 ; became quieter after the pediluvium, and had some sleep ; pulse just now 136, soft, of moderate strength ; heat $102\frac{1}{2}^{\circ}$; tongue moist on the edge ; coughs a good deal ; breathing easy ; no pain ; occasional delirium ; has tremors, but no subsultus ; bowels open.

Habeat vini rubri uncias vi. Continuentur alia.

14th.—Much restlessness and delirium yesterday evening ; became quiet and slept after the pediluvium and draught ; no stool ; no sweating since yesterday ; had last night eight grains of antimonial powder, with two of calomel ; pulse 144, of moderate strength ; tongue rather dry ; heat 102° ; slight cough continues ; no dyspnoea ; no pain ; has still deafness, slight subsultus, and occasional delirium ; countenance pretty natural.

Habeat Olei Ricini semunciam, et repetatur tribus horis exactis nisi soluta fuerit alvus. Continuentur alia.

15th.—Five stools ; has had much restlessness and delirium with tremors and subsultus, but slept pretty well after the opiate ; pulse 134, very soft ; heat 102° ; still some cough ; no pain ; tongue dry and tremulous. A blister has been applied to the nape of the neck ; eight ounces of wine and a bottle of porter taken ; some inflammation over the sacrum and hips.

Foveantur membra inferiora sæpius ; Repet. haustus hora somni ; habeat vini uncias viij.

16th.—Pulse 132, very soft ; heat $103\frac{1}{2}^{\circ}$; one scanty stool ; tongue moist ; is still quite incoherent and restless, with tremors ; sleeps a good deal at times, and takes some food ; less cough.

Habeat vini uncias x. ; Continuentur alia.

17th.—Pulse 126 ; tongue rather moister ; heat 101° ; slept after his draught ; still very incoherent ; two stools.

Continuentur vinum, cerevisia, mistura, et haustus.

18th.—A pretty good night after the draught ; pulse 120, of better strength ; tongue dry ; heat 99° ; has taken 16 ounces of wine, and nearly a bottle of porter, with some food : countenance very pale and languid. Is still incoherent, but less restless ; more moaning and tendency to coma ; still very deaf ; little subsultus ; no stool to day.

Habeat vini rubri libram ; continuentur alia ; et habeat haustum ex Oleo Ricini.

19th.—Eighteen ounces of wine and a bottle of porter taken ; had threestoools from the castor oil ; pulse 114, of moderate strength,—more frequent and smaller an hour ago before he took some of his wine ; heat 99° ; tongue moister ; still much delirium, but he lies

pretty quiet ; little food taken ; a little ulcer over the sacrum, with gangrenous edges.

Continuentur medicamenta, vinum, et cerevisia.

R *Sulphatis Quinæ, Pulveris Aromatici, singulorum grana iiii.*

M. fiat pulvis, ter indies sumendus. Applicetur Unguentum Resinæ ulceri dorsi et cataplasma emolliens.

20th.—Had a quiet night ; is still incoherent, but not restless ; has taken sixteen ounces of wine, and a bottle of porter, and eight ounces of the camphor mixture ; a good deal of tremor at times ; pulse 112, rather small ; heat 93° ; tongue moist ; very little food taken ; three stools ; ulcer on sacrum as yesterday.

Continuentur vinum et cerevisia. Intermittatur haustus sedativus. repetatur mistura camphoræ.

21st.—Two stools since yesterday ; little food taken ; has slept much ; is still quite incoherent and very deaf ; pulse 108, rather small ; heat 98° ; tongue dry ; fourteen ounces of wine and twenty-four of porter taken.

Continuentur medicamenta ut antea.

22d.—Slept much and is less incoherent ; pulse 96, of moderate strength ; deafness and tremors continue ; tongue dry ; two stools ; part of the gangrenous edge of the ulcer has sloughed off ; gangrene not extending.

Habeat vini uncias x; continuentur alia.

23d.—Has slept much ; very little delirium ; pulse 90 ; tongue moist ; more food taken ; still slight subsultus ; one stool.

Continuentur medicamenta.

24th.—Sleeps much ; still occasional slight delirium ; pulse 96, rather small ; tongue moist ; ulcer on the back improving ; two stools.

Continuentur medicamenta.

25th.—Sleeps much and quietly ; pulse 90, of moderate strength ; tongue rather dry ; two stools ; tremors and subsultus gone, but he has still occasional delirium ; more appetite.

Habeat vini uncias vi. ; intermittatur mistura ; continuentur alia.

He got gradually better, but was not dismissed cured till the 4th June.

ART. II.—*Case of Gonorrhœal Ophthalmia.* By J. H.

WISHART, F. R. S. E. Surgeon to the King in Scotland.

ON the 21st June 1826, I was requested to visit Mr D. A., a young gentleman about 25 years of age, who had been affected with severe ophthalmia of his left eye of about six days' continuance. He had great intolerance of light, severe pain and lachrymation, with copious puriform discharge. There was also chemosis, especially of the conjunctiva of the lower segment of the eye-ball, causing depression of the lower eyelid ; both eyelids were very turgid. He had had gonorrhœa for about

three weeks ; the discharge still continued ; he had been freely bled, both generally and locally.

On the 22d there was slight opacity of the lower part of the cornea, and considerable turgescence of the lower segment of the conjunctiva. A circular portion of it was snipped off with the curved scissors ; the cut surface bled freely. He was ordered pills of a grain of calomel, and two of hyoscyamus night and morning ; the eye to be bathed with a weak solution of the muriate of mercury.

On the 23d the cornea had partially given way ; it was opaque, and ulcerated. The iris had fallen forward.

Extract. Belladon. gr. i. ter indies. Sulph. Magnes. cr. m.

R. Pulv. Ipecac. et Opii gr. x. Pulv. Antimonial. gr. ii. M. h. s. s.

24th.—Slept tolerably well ; much less discharge from the eye during the night ; it has been bathed frequently with the tepid solution of mercury ; swelling of the conjunctiva diminished ; cut surface discharging freely ; opacity and ulceration of the cornea appear to be stationary ; no pain ; rather less intolerance of light and of lachrymation. Salts have not operated.

Evening.—Salts have only operated once ; ulceration of cornea rather extended, and a portion at the lower part covered with a slough ; touched with the lunar caustic ; pulse 84, rather fuller ; discharge moderate ; slight tenderness of right eye.

Sumat h. s. Pil. Coloc. c. ii. et. Cont. Pil. Submur. Hydr. et Belladon.

22d.—Passed a very good night ; right eye much more affected ; there is considerable increase of the vascularity ; the conjunctiva of a very pale red colour ; very slight appearance of chemosis at the lower segment of the eye-ball ; considerable exudation of thin puriform matter ; edge of eyelids very red ; complains of considerable pain and tenderness of the eye on being touched, and of pain along the margin of the orbit ; intolerance of light ; pulse 120, full and strong. Left eye much as yesterday ; ulceration has not extended ; slough of the cornea has not yet separated. Bowels have been freely opened.

F. V. S. stat. ad 3xv. et post horas tres appl. Hirud. viij fronti. Sum. Solut. Tart. Antimon. omni bihorio. Diet, bread and water ; a large poultice to be kept applied to the perineum.

Evening.—Was much relieved by the bleeding ; full quantity taken ; blood cupped, and slight buffy coat on one or two of the cups ; pulse 92, still full, and of good strength ; right eye looks rather favourably ; less appearance of puriform discharge, and he does not feel any pain except when he moves the

lids; left eye unchanged; a portion of the conjunctiva of the lower part of the eye-ball snipped off.

Although his pulse was much reduced in frequency, as it was still full and strong, it was deemed advisable to take again fifteen ounces of blood from his arm, and to apply a large blister behind the right ear, to increase the calomel to two grains night and morning, to repeat the Dover's powder at bed-time, and the sulphate of magnesia in the morning. The eye was directed to be frequently bathed during the night with a mixture of vinum opii and elder flower water, the left eye with the solution of the muriate of mercury.

26th.—Passed rather a restless night, but not from pain of the eyes; pulse about 96, not so full, and much softer; complains of pain only when he moves the eyelids, which are much less swelled; edges of them red and tender; discharge much lessened; right eye, pupil and cornea quite bright and natural; cut surface of conjunctiva discharging considerably; puriform discharge much less; cornea of left eye still very opaque, and ulceration seems to extend to the upper segment of it; the general obscurity prevents the state of the internal parts being ascertained; discharge also lessened.

Cont. pil. Calomel. et Belladon. Solut. Antimonii in M. salin.—Pulv. Ipecac. et Opii, h. s.

27th.—A good night; right eye feels much easier; he opens it so freely as to allow the cornea to be distinctly seen, and he has scarcely any pain on moving the eyelids, the swelling of which is much diminished; redness of conjunctiva much paler; discharge from the cut surface still continues; it is quite different in appearance from the diseased discharge; cornea and pupil quite natural; left eye stationary; corneal slough not separating; no pain; discharge less; pulse 84, soft, and of moderate strength; bowels opened twice; mouth slightly affected.

Cont. lotio vin. Opii et Aq. flor. sambuc. et applic. frigid. Lavet. ocul. sinist. solut. e nitrat. argent. gr. v. in Aq. distill. ʒi.—Cont. alia.

28th.—Continues improving; a good night; pulse always accelerated in the morning, probably owing to the sun beating on his bed room in the early part of the morning; it becomes nearly of natural frequency towards evening; right eye still better; redness of vessels diminished; very little discharge except from the cut surface of the conjunctiva; corneal slough of the left eye beginning to separate; still great obscurity; redness and fulness of vessels abated; no pain.

Lavet. ocul. dext. Collyr. e sol. acet. zinc. tenuior. ʒx et vin. opii ʒii.—Lavetur ocul. sin. ut antea sol. nitrat. argent.; Cont. Pil. Hydrarg. et Belladon. et M. salin. antimonial.

To be allowed a little more nourishment.

29th.—The vinum opii was applied to the eyes last night ; it smarted both very much, but after the pain abated they both felt very comfortable ; had a good night ; inflammation of both eyes sensibly subsided ; opens them both quite freely ; puriform discharge nearly gone ; pulse natural ; feels very weak ; mouth sore from the mercury.

Intermitt. Pil. Submur. Hyd. et Bellad. Contin. Vin. Opii et Pulv. Ipecac. et Opii, et Pulv. Antimonial. h. s. Utat.

Collyr. Op. et Samb. et sol. Nitr. Argent. ut heri.

30th.—Right eye going on favourably ; number and size of red vessels greatly diminished ; slough begins to separate from the cornea of the left.

Sum. Pil. e Sulph. Zinc. gr. iii. ter indies. Contin. Collyria.

To be allowed chicken soup, beef tea or pudding.

July 2d.—Eyes continue to improve ; inflammation nearly gone ; scarcely any discharge from the right.

Contin. Vin. Opii et Pil. Sulph. Zinc. Applic. ocul. sinist.

Ung. oxid. Hydrarg. rub.

7th.—Going on favourably ; redness of right eye confined to the internal angle ; cornea of left eye now so cleared up that it allows the pupil to be seen ; iris in contact with it all round, and projects through the ulcer at the lower part. Continue.

14th.—Right eye free from inflammation, but very weak ; left eye much improved ; cornea becoming more prominent ; and anterior chamber re-forming ; iris adheres to the cornea, and projects through the ulcer ; vision daily improving ; can distinguish the pictures on the wall ; surface of the cornea abraded nearly over its whole extent.

Cont. Vin. Opii et Ung. ox. Hydr.—Applic. Nitrus Argent. hernice irid., altern. dieb.

18th.—Projecting part of the iris punctured ; aqueous humour evacuated ; the prolapsed iris collapsed into contact with the cornea ; right eye still improving.

Cont. Vin. Opii. Omit. Nitr. Argent.

20th.—Iris now on a level with the edges of the ulcer of the cornea, and slightly covered with lymph.

Cont. Vin Opii m. et v. Solut. Bel. ter indies.

30th.—Left eye continues to gain strength ; the concavity of the cornea at the inner angle diminishes ; vision much improved ; less opacity around the edges of the ulcer.

Cont. Vin. Opii, Solut. Belladon.—Utat Collyr. e Lap. Divin.

Di in Aq. 3vi. soluto.

August 6th.—Convalescent.

To omit the Belladonna and continue the *Sol. Lapid. Divin.*

This patient called on me about the beginning of the year

1827. The vision of the right eye was perfect. In the left there was adhesion of the iris to the cornea, with a depression on the cornea at the upper part; the vision of this eye was tolerably good, and there was scarcely any perceptible deformity.

The Gonorrhœal Ophthalmia begins at uncertain periods after the patient has been affected with the disease in the urethra. In the cases which have come under my notice it has always commenced during the inflammatory stage. The patients have invariably been stout young men of a plethoric habit of body.

From the observation of some writers, it would appear that in some cases the inflammation of the urethra completely disappears while the ophthalmia develops itself. It has therefore been divided into three species; 1. ophthalmia from an accidental translation of the matter to the eye by means of the fingers, or the application of cloths soiled with the urethral disease; 2. ophthalmia from suppressed gonorrhœa; and, 3. ophthalmia, the discharge still continuing from the urethra.

The gonorrhœal ophthalmia appears, after the patient has been exposed to the influence of such causes as are capable of stopping the discharge from the urethra, as a violent degree of inflammation excited by the incautious use of internal stimulants during the inflammatory stage of the disease, or by the use of strong astringent injections, by exposure to cold, or the use of cold fomentations.

The course of the gonorrhœal ophthalmia is marked not only by the rapid extension of the inflammation from the lids to the eyeballs, but by the state of chemosis of the conjunctiva of the eyeball, and the rapid destruction of the anterior membranes of the eye. During the whole progress of the disease we never meet with any symptoms of inflammation of the iris; thus showing the distinction between this species of ophthalmia and the ophthalmia from true syphilis.

The gonorrhœal ophthalmia begins with inflammation of the edges of the eyelids, which appear red, tense, swelled, and painful. The swelling quickly extends to the inner surface of the eyelids. In the course of from twelve to forty-eight hours the disease reaches the conjunctiva covering the sclerotic coat, which rapidly swells and passes into the state called chemosis. The swelling of the conjunctiva is raised like a wall over the edge of the cornea; it is tense, of a deep red colour, accompanied with violent pain and intolerance of light, and acute inflammatory fever. The swelling of the external parts of the eyeball is also attended with considerable tumefaction of the lids, especially of the upper eyelid.

This ophthalmia is accompanied with a most profuse dis-

charge of puriform matter. It begins in a very short time after the attack; the matter resembles that of gonorrhœa in its appearance; it is at first white, then becomes yellow, and is latterly of a greenish colour. As the disease advances the pain rather abates; the swelling is greater but less tense and less sensible; and the general febrile state diminishes. The secretion from the eyelids increases and rapidly extends to the conjunctiva of the sclerotic coat, and the secretion is so abundant that it runs over the cheek.

When the disease has reached this state, ulceration of the cornea rapidly follows. This differs from all other ulcers of the cornea which are funnel-shaped and filled with pus, whereas in this ophthalmia they are extensive, unequally deep, and seem exactly as if a bit of the cornea had been torn away with a hook. At the same time a muddiness diffuses itself over the rest of the cornea, and the matter effused between the layers of it soon extends over the whole, and gives the cornea the appearance of being swelled and thickened. These abraded parts speedily give way, and the surface of the cornea resembles a mass of pus; perforations take place at several places, and prolapsus of the iris follows. After this the disease in general abates, but in some cases the destruction goes still farther, the cornea bursts more extensively, and the lens and part of the vitreous humour are discharged, and the eyeball collapses.

The event of this disease is in the greater number of cases most unfortunate; and of various instances that have come under my own notice the case above related is the only one that has come to a favourable termination. If the surgeon were fortunately called at the very commencement of the disease, before the beginning of the secretion from the lids and eyeball, he may expect to be able to save the eye and restore the sight.

The swelling of the conjunctiva of the eyeball diminishes in the same way as it increased, and remains longer in the lower half of the eyeball, and the chemosis gradually disappears. The inflammatory swelling of the lids remains still longer, and frequently degenerates into the granular state of the lids, as after the Egyptian ophthalmia. If the surgeon is not called in till after the secretion of the puriform matter has commenced, there is little hope of saving the eye, unless very active measures are used. We meet with cases, however, as in the left eye of the gentleman, the subject of this essay, where some benefit may be obtained, and where the form and even a tolerable sight of the eye may be preserved. In these cases, as the disease abates, we find that the matter effused into the cornea has left partial opacities of it, or slight prolapsus of the iris.

Another of the sequelæ of this inflammation, after the total

loss of the sight, is the partial absorption or thinning of the sclerotica, in consequence of the ulceration of the edge of the cornea having extended to this coat, which cannot be easily ascertained during the profuse discharge from the eye. The choroid coat and a portion of the ciliary system shine through, and sometimes even a protrusion takes place, forming a dark bluish tumour, which has been called staphyloma of the sclerotica.

In the treatment of the gonorrhœal ophthalmia it has been long a subject of controversy, whether it is proper to endeavour to recal the discharge from the urethra or not. While some writers have strenuously recommended it, others have been equally keen in condemning the practice, asserting that the disease is to be cured by the strict antiphlogistic regimen, and the removal of the surface secreting the matter discharged from the eye. In the cases which have come under my own observation, the discharge from the urethra has never been completely stopped; but if a case of complete metastasis of the inflammation from the urethra to the eye were to occur, and if there was no remains of inflammation of the urethra, I would be induced to adopt the measure of inoculation by the matter from the eye, or the introduction of a bougie covered with some stimulant, to endeavour if possible to recal the discharge from the urethra.

With regard to the treatment of the gonorrhœal ophthalmia, it has been already remarked that the patients are in general full and plethoric, and consequently the antiphlogistic regimen, in the strictest sense, ought to be adhered to. A free bleeding from the arm to the extent of twenty-five to thirty ounces should precede the use of purgatives and cooling drinks, with the free use of tartar emetic in small doses, repeated at intervals, to keep up a constant state of nausea. The diet should be confined to bread and water, fruit or vegetables. The feet may be put into warm water, and an emollient poultice should be kept applied to the perineum.

The local treatment of the gonorrhœal ophthalmia must be directed to the removal of the immense discharge from the eye, and the preventing of its accumulation. In the case just detailed very evident benefit arose from the snipping off a portion of the conjunctiva of the eyeball, and it is only to be regretted that in the left eye it had not been done a day or two earlier. It had an immediate effect in diminishing the quantity of the secretion, and in altering its character; it became thin, and of a different colour. From observing this effect on the left eye, excision was adopted at a very early period in the right, almost before the state of chemosis of the sclerotic conjunctiva

appeared. This practice has been strongly recommended by Spangenberg and Wendt, with the view of rendering the diseased organ secreting the mucus, viz. the swollen and projecting conjunctiva, incapable of doing so, by cutting out a portion of it with the curved scissors, and allowing it to bleed freely. If the cornea has already become ulcerated, the excision of the conjunctiva cannot cure it, but it does not seem to have any bad effect on it.

In the gonorrhœal ophthalmia the weak solution of the muriate of mercury is recommended as the best collyrium for washing away the matter from the eye. It requires to be used every hour in many cases. It is most conveniently used with a bit of fine sponge, the eyelids being carefully separated with the one hand, while the liquid is dropped in with the other. It may also be of advantage to use the small syringe, where there is very great tumefaction of the eyelids. After the conjunctiva has been cut out, I consider it preferable to use a mixture simply of vinum opii and elder flower water, made warm. The parts surrounding the eye ought to be always carefully dried after using the eye-water, as any cold evaporation I conceive to be hurtful. The pure vinum opii is to be used as soon as general and local bleeding have subdued the violence of the inflammation. In this stage blisters to the nape of the neck, or to the mastoid process, are of manifest benefit. In general it is fully preferable to apply a succession of blisters rather than to keep them open. The precipitate ointment may be used on the alternate days at bed-time, and when the cornea has ulcerated, the best local application is the solution of the nitrate of silver, from three to five grains to the ounce. When the iris projects, it requires to be touched with the solid cone of caustic; and in cases where it seems not to be acting quickly, I have found great advantage from puncturing the little projection with a cataract needle; the aqueous humour is discharged, the tumour collapses, and is constricted by the edges of the ulcer of the cornea.

The internal use of mercury has been recommended by some writers, and by others it is considered of no use. When given in small doses, combined with the extract of hyoscyamus, it may be of advantage in promoting the absorption of the matter effused into the layers of the cornea. The benefit derived from it is not so marked as in the syphilitic inflammation of the iris.

York Place, July 1827.

ART. III.—*On the Use of Hydrocyanic Acid in Chorea.* By
JOHN STUART, Surgeon, Kelso.

MY attention was first directed to the use of the Prussic acid by the work of Dr Granville. From the high encomiums of that gentleman with regard to its utility in pulmonary complaints I was induced to give it a trial; but I was much disappointed, having perceived little or no benefit from it whatever in these cases. For two or three years after I ceased to use it; but about twelve months ago I was led to give it another trial in a young lady, labouring under the case of an affection of the spine, who was tormented with most distressing nervous symptoms. To relieve these the various anodynes were had recourse to, but with little benefit, as the constipation they produced more than counterbalanced their calmative effects. At last I thought of the prussic acid, and I was delighted with the result. It allayed in a remarkable manner the irritability of the nervous system. She took during her illness, of which she died, upwards of eight ounces in six months.

From its utility in this case I was subsequently led to try it in two cases of Chorea. The reports of these cases I herewith send you, and if you think them at all interesting, you are at liberty to make what use of them you please. I am aware of the impropriety of drawing conclusions from the results of two cases; but as chorea is a disease one seldom meets with, it may be long before I have an opportunity of trying the remedy again; and, in the meantime, I should be happy that it were put to the test by others of the profession.

The purgative system, recommended by Dr Hamilton Senior, I believe to be eminently successful in the majority of cases, and the two subjoined might have yielded to it. At the same time, there are cases which certainly do not yield to that plan of treatment, and in such the acid might be had recourse to. In both the following cases purgatives were tried freely, though perhaps not for a sufficient length of time. In the case of Sally Younger the bowels were so completely emptied, that there seemed to be nothing left in them,—the spine could be felt through the parietes of the abdomen,—the alvine evacuations had become perfectly natural, both in appearance and smell; and yet the disease gained rather than lost ground up to the very day the acid was first given. Forty-eight hours after this, an amendment was observable, and it continued progressive until the cure was complete. In every instance, however, I think free purging should first be employed, and continued as long as the matter passed is unnatural in any respect; for

if the disease be owing to a loaded state of the *primæ viæ*, no medicine acting on the nervous system alone can be expected to do any good.

I tried the acid also in the case of a woman who, for several years, has been affected with an organic disease of the uterus. She has not menstruated for a very long period, but every month she is regularly attacked with violent pains in the uterine region, accompanied with a great bearing down sensation, so very great that a person entering the room would fancy she was about to be delivered of a child. To alleviate her sufferings she used to take large doses of opium or laudanum, both in draughts and by injection, frequently with little relief, and always with the effect of inducing great sickness at stomach, to avoid which she frequently declined taking any anodyne. I gave her the acid in doses of six drops, which producing considerable relief, I ventured on giving it in doses of twelve drops, and with the greatest success. It arrested completely in the course of twenty minutes the uterine action. She now takes it every time she has an attack, and very seldom is a second dose required. She now feels as well the day after an attack as she used to do at the end of three or four days, or even a week. From this effect on the uterine system, might it not be employed in those cases where it is thought advisable to suspend for a while the uterine pains? If such a case should occur to me I would not hesitate in giving it a trial.

To insure the successful exhibition of this medicine several circumstances must be particularly attended to. Care should be taken that it is genuine. What is purchased in the shops is often far from good.* The best way to give it, I believe, is in simple distilled water; or, if it should be apt to excite nausea, in cinnamon or peppermint water; and on account of its great liability to spontaneous decomposition, the mixture should never be made to last longer than twelve hours. I am perfectly satisfied that it often becomes quite inert after standing a longer period. The interval, too, betwixt the doses should not exceed two hours; for the effects of the acid are almost immediate and very transitory; and it is of consequence, in the cure of chorea and other nervous affections, that its effects on the system be never allowed to go entirely off. In the spine case above alluded to this was so very evident, that the mother of the child never scrupled to awaken her in order to give it, as she found from experience, that, if she allowed a longer time to elapse, the full effects were not obtained again for a considerable time.

* It may be had, however, very excellent from Duncan and Ogilvie, Edinburgh, and from Dr Nimmo of Glasgow, who prepares it after a process of his own.

Case I.—October 19th, 1826.—Janet Reid, aged 13, about fourteen days ago began to complain of pains and weakness of the legs, arms, and back. An unsteadiness in walking was at the same time observed, which gradually increased until she was unable to walk at all. I visited her to-day, and found the gesticulations of the legs and arms very strong; and indeed the whole body was convulsed. Speech indistinct; swallowing difficult; pulse 90; bowels costive; has taken opening medicine several times; has never menstruated. The motions of the body never cease, excepting when she is asleep; awakes always in a great fright.

To have a mercurial purge (calomel and jalap) every other day, with senna tea, till the bowels be freely opened.

October 27th.—The powders operated freely; matter passed offensive, but she is no better; gesticulations as violent as ever; face and breast bruised from the blows she gives herself; speech quite inarticulate; appetite impaired.

R. acidi Hydrocyanici guttas xxvij. Mucilaginis gummi arabici, et aquæ cinnamomi, singulorum unciam. Misce. Two tea-spoonfuls to be taken every second hour.

November 3.—Has taken four drops of the acid every two hours since last report. She appears much relieved; gesticulations much less violent; speech more distinct.

Continuetur mistura ut antea.

5th.—Continues to improve; gesticulations much abated; bowels costive; belly very flat.

Sumat acidi Hydrocyanici guttas sex alternis horis; et pilulas gambogiæ duas quotidie hora somni. R. Olei Camphorati uncias ii., Opii drachmas ii. solve. Fricetur dorsum ope hujus linimenti.

8th.—Continues to improve.

Continuetur acidum hydrocyanicum.

9th.—Not having been in the habit of making up the mixture myself, I committed a mistake, and gave her yesterday double the quantity of acid. Anxious to know the result, I visited her early this morning, and was pleased to find that she had had a very good night's rest, having slept calmly till three o'clock in the morning, and she felt no unpleasant effects from the largeness of the dose; on the contrary, she expressed herself much better. The motions of the legs have almost entirely ceased, and those of the arms are much diminished. The muscles of the face are still much affected, occasioning strange grimaces. She has much more command of the tongue, being able to thrust it out freely, and to speak much more distinctly. Bowels kept gently open by the pills, and the matter passed is healthy in appearance; appetite much improved. I ordered

her to go on with the pills every night, and, the weather being very fine, desired her to be taken out every forenoon in a cart, and to continue the acid every two hours in the dose of eight drops.

November 10th.—Within these forty hours has taken ninety-six drops of the acid, and to-day her mother says she is still continuing to improve. Does not feel any unpleasant effect from the acid whatever, excepting a little nausea. Says it produces on being swallowed a great heat all over her, followed by profuse perspiration. As she now nauseates the acid mixture, I ordered eight drops to be given in cinnamon water every two hours while she was awake, and as long as no unpleasant symptoms should arise.

12th.—The acid has occasioned slight headach and profuse perspiration; she is now able to hold things in her hands, and to carry them to her mouth; sleep very composed; slept last night ten hours; appetite much improved; bowels kept open by taking one aloetic pill every night; back rubbed every night with the liniment.

To continue the acid, but only in half the dose.

14th, 18th, 24th.—She is now much better; she is now to discontinue the acid, and take of a solution of quina in wine a teaspoonful twice a-day in a little water.

December 3d.—Since last report I have received no intelligence of her from her friends, but I understand she is now doing well, and is able to go about.

10th.—She is now quite well, and has got very fat and stout.

Case II.—April 5th, 1827.—Sarah Younger, aged 13, about four weeks ago was first observed dull and not in her usual health. Her walking then became unsteady, and at last she entirely lost the use of her legs and arms. She has been confined to bed for some time. Her whole body is now in constant agitation; sleep very disturbed; awakes always in great alarm, fancying she sees frightful objects. The abdomen feels quite flat; has taken two mercurial purges, and doses of senna tea occasionally; bowels open, and what she passes is very offensive; appetite pretty good; thirst urgent; urine in usual quantity; pulse not affected; has never menstruated.

℞ *Submuriatis Hydrargyri grana iv., pulveris Aloes Socotorinæ grana vi. Misce. Fiant pulveres hujusmodi iij.*—*S. One to be taken every other day, and a little infusion of senna on the intermediate days.*

April 13th.—Continues much in the same state; gesticulations as violent as ever; tongue more affected, rendering her speech very inarticulate; complains frequently of violent pain

of the belly ; tongue not very foul, much injured by her teeth ; bowels have been well purged with the powders, and what she now passes is quite natural in appearance, and not offensive in smell ; has very frequent desire to make water ; passes it with great difficulty, and in small quantity.

To continue the purgative medicine.

16th.—No improvement whatever ; has continued taking laxatives every day, which operated freely. The abdomen is now quite flat. The medicine to be continued as above.

20th.—She is no better to day ; gesticulations as violent or rather more so than ever ; she is indeed with difficulty retained in bed ; purging system pursued with vigour up to this date ; alvine evacuations quite healthy. The abdomen is now so flat and empty that the spine can be felt through its parietes. Seeing no relief from the usual treatment I resolved to try the prussic acid. She began by taking two drops every two hours in a little peppermint water, and every day increased each dose by a drop.

21st.—Has taken eight doses of the acid. The first dose made her very sick, and caused violent vomiting, but the succeeding doses only occasioned uneasiness of stomach and bowels, and headach. Gesticulations not diminished, and in other respects she is much as before.

24th.—I found her to-day considerably calmer ; motions of the body not nearly so violent ; says she feels better ; has taken the medicine regularly every two hours, and the dose was increased to three drops ; but it then occasioned violent retching, upon which it was again reduced to two drops. On account of the inconvenience of giving it every two hours during the night, it was ordered not to be given till she awoke in the morning, and then to be continued through the day as before, and increased, if possible, by two drops daily. Appetite and state of bowels as before.

27th.—No improvement ; bowels have become constipated ; to continue the acid, and to take two gamboge pills occasionally to keep the bowels open.

29th.—Continues much the same ; bowels now regular ; matter passed healthy. The acid to be continued.

May 2d.—The gesticulations are becoming less violent ; speech much the same ; sleep not so disturbed ; bowels open. In consequence of a mistake on the part of her attendant the acid was not given at the usual periods, and when it was commenced again it occasioned uneasiness of stomach, with violent retching as at first. I ordered that she should continue the acid and pills, and the weather being fine, that she should be carried out daily to get the air.

4th and 9th.—Continues to improve.

15th.—I found her to-day very much better. The gesticulations are much abated, particularly of the legs; speech also more distinct; is able, with a little assistance, to walk across the room; appetite not so good this week; bowels more constipated; has taken a laxative pill every other night, and the acid in doses of three drops and a-half every two hours. Her mother says her stomach does not retain it so well as it did, and particularly on the days she takes the pills. I desired her to continue the acid in such quantity as the stomach might bear, and to keep the bowels open.

17th.—Continues to improve slowly; motions of the arms, however, are still considerable, and speech inarticulate; stomach will scarcely bear the acid at all. I ordered the acid to be gradually discontinued, and a wine glassful of bitters to be taken twice a-day, and the pills as before.

20th.—Continues to improve; is now able to walk to the door without assistance, and she can make use of her hands; speech also more articulate; appetite improved; bowels regular.

To discontinue the acid altogether, and to go on with the bitters and pills.

27th.—Convalescent; to continue the pills, and use the cold bath daily.

ART. IV.—*Further Observations on Intermittent Fever, in reference to the beneficial effects of Blood-Letting in the Cold Stage.* By JOHN MACKINTOSH, M. D., F. R. C. S., Acting Surgeon to the Ordnance in North Britain; Physician to the General Dispensary, Brown's Square; and Lecturer on the Practice of Physic, &c. in Edinburgh.

MY first communication on intermittent fever* may have been thought deficient, from the absence of all reasoning on the subject; but I considered it most expedient to state the facts as they had occurred to me, and to leave them before the profession for some months, together with such conclusions as I thought myself warranted in drawing. I expected considerable opposition and much special pleading from the numerous staunch disciples of the Cullenian system of medicine, merely because it is natural for the human mind to cling to the authority under which it has been fostered and matured.

The object of this communication is to obviate the objections which may be urged against the practice of bleeding in the cold stage, and to adduce my reasons in its support.

* Inserted in vol. xxvii. of this Journal.

1st, The grand objection to bleeding in the cold stage of intermittent, viz. That it will inevitably produce death, must henceforth be abandoned. Cullen's system did not indeed create this objection ;* but his dogma of "debility, diminished energy of the brain, and spasm of the extreme vessels" being "the proximate cause of the cold stage," gave the erroneous assertion so much weight, that, even after perusing the facts recorded by me, and the public manner in which the trials were made, some highly respectable persons still think that some mistake has been committed. Let such gentlemen reflect upon the practice they would themselves pursue, if called to a case in which congestion of the brain had taken place to such a degree as to produce all the symptoms of convulsions or of apoplexy, or in which congestion of the lungs had caused a near approach to asphyxia. They would not hesitate to open a vein even when there is no heat of surface, and a pulse by no means strong, perhaps so weak as scarcely to be felt at the wrist. I beg they will also recollect the practice pursued by our brethren in the East Indies in the worst forms of cholera. It appears, that, if the practice is had recourse to in proper time, if, in fact, they can put the blood once more in motion, the patients have the best chance of recovery. In such extreme cases powerful

* In my former communication I have erroneously attributed to Celsus the assertion that *bleeding in the cold stage is death*. On a careful reference I find no allusion made to such a practice in his works ; but in treating of the danger of bleeding in *vehement fevers* he expresses himself thus,—"*Quod si vehemens febris urget, in ipso impetu ejus sanguinem mittere, HOMINEM JUGULARE EST.*" Lib. ii. cap. 10. I was led into this error, which I am glad to acknowledge and correct, by a statement made to that effect by the late Dr Gregory in his Lectures. "I have no doubt, therefore," (said he,) "that the causes producing fever act first by inducing debility, and accordingly we find that stimulants employed at this period have often produced good effects in checking the disease, while evacuations, as *blood-letting*, which at another period of the disease might have been proper, if employed in the *first stage*, never fail to be attended with most dangerous consequences, or it is, to use the words of Celsus, '*hominem jugulare.*' It appears to me that this justly celebrated individual was himself misled by a statement made by Sir John Pringle, who, in allusion to the good effects of bleeding in the camp fever which he describes, observes at page 210, (*Observations on Diseases of the Army*, Ed. 1768,) "A person unacquainted with the nature of this disorder, and attending chiefly to the paroxysms and remissions, would be apt to omit this evacuation, and to give the bark prematurely, which might bring on a continued inflammatory fever. A vein may be safely opened either during the remission or *in the height* of the paroxysm ; for besides that I have observed the remission to come sooner and fuller after hemorrhage, I have repeated experience of the safety of bleeding in the hot fits ; and not only in this, but in the marsh fever, even after it had come to almost regular intermissions. In order to make Celsus's maxim (he quotes the above passage from lib. ii. cap. 10.) consistent with this practice we must interpret his terms *impetus febris* in the sense of that chillness or cold fit which preceded the hot one in the fevers which he describes, *for then bleeding would indeed be improper.*" This is straining an author's statement to suit other views with a vengeance. The meaning of Celsus is clear and precise,—he makes no allusion to the cold stage. If the reader will refer to his chapter on the cold stage of intermittents, no allusion whatever will be found to bleeding. This shows how likely we are to be misled by the authority of a name.

stimulants may be administered to raise the heart's action for the moment, while the bleeding unloads the organ, and enables it to continue its contractions with sufficient energy to pump the blood to the surface.

If it be said that these are not cases of intermittent fever, —that the causes, the symptoms, the course, and terminations, are different; my answer is, that the pathological eye can see connections and similarities, which one guided by mere symptoms will never be able to discover, or perhaps believe, after they are discovered. To such persons in the profession as have “pinned their faith to the sleeve” of Cullen, I shall say nothing in this paper. On a future occasion it is my intention to analyze Cullen's doctrines regarding fever, with a view of showing how erroneous and unphilosophical they really are. If my time will permit, I shall then bring before the profession some views which, in my humble opinion, are not only better calculated to explain the phenomena of fevers, but which will also in a great measure, account for the discrepant histories that have been handed down to us, and the different success of opposite modes of treatment. It may be proper for me in this place to make a short declaration of my faith. I am not one of those who think that the combination of symptoms denominated fever is invariably produced by inflammation of one viscus, or set of viscera, or that fever and inflammation are synonymous terms.

The success of bleeding in the cold stage of intermittent fever is a new and an important fact in the practice of medicine. It is applicable not only to intermittent fever, but to the first stage of all other forms of fever. I have frequently put it in practice myself in the common fever of this country; but it has been employed much oftener by my dispensary pupils, and always hitherto with marked success.

Two men were affected with similar symptoms, indicative of the first stages of fever. They were both young men, and had been living pretty much alike, and breathing the same air. Both complained of languor, debility, headach, &c.; and both had frequent chills and flushes of heat. One whom I bled during a chilly attack was convalescent on the third day. The other case, which was then by far the slightest, was treated in the ordinary manner. He was six weeks confined to bed, during which time his life was several times in jeopardy, from pulmonary, cerebral, and gastro-intestinal inflammation. He was delirious; picked the bed clothes; passed urine and feces involuntarily in bed, and was covered with petechiæ. He was indeed cured at last, but at four or five times the expence of blood,

without taking into account the local bleedings by cupping and leechings.

2dly, But, in the second place, it may be said, that although bleeding in the cold stage does not actually produce death, its immediate or ultimate effect must prove debilitating.

Before appealing to the facts as they have occurred, I may be allowed to mention that it is a common, and often a fatal error in medicine to mistake mere oppression for debility. In an ordinary cold stage it is not actual debility, but general oppression, which takes place, partly from the overloaded state of the heart, but also from the functions of many organs becoming more or less impeded in consequence of venous congestion, and likewise, I have no doubt, from the want of a sufficient supply of proper arterial blood. In all the cases which I have seen, the immediate effects of bleeding were not only not debilitating, but decidedly invigorating; and even in some few instances in which the bleeding produced a tendency to syncope, the feeling of debility was only slight and transitory. The remedy seems to have an invigorating effect merely by removing the oppression. It restores the lost balance of the circulation, and also removes the venous engorgement from viscera, whose functions are necessary to life. I have myself been surprised at this sudden change from apparent debility to actual strength, the pulse from being weak, becoming strong, even in old standing cases, in which the constitutions of the individuals were materially injured by repeated attacks of the disease. I shall merely refer to the cases lately published in this Journal.

It will be observed by glancing at Case I. (that of Ward,) that he had been affected several times annually for fourteen years, and that his constitution appeared to be much broken down; but his strength began to improve from the moment he was bled in the cold stage. I had daily opportunities, for some months afterwards, of seeing this man, and, as my report states, "his constant tale was, that he had not felt so well, or so much of a man since he went to Walcheren," where he was first attacked when serving with the expedition in 1809.

The patient treated by Assistant-Surgeon Marshall, (Case VII.) deserves particular attention. The health of this individual when he was blooded by Mr Marshall was so much impaired by repeated attacks of ague, that he was, in the first instance, sent home from Jamaica, and he was ultimately discharged from the service, having been for some time dropsical. Even in this unpromising case the cure seemed to be as sudden and as permanent as that which took place in Ward's case.

In the eighth case it will be perceived how much the patient's constitution had been injured by the disease. He represented his

strength, which had been previously very great, to be reduced to the weakness of a child. The report speaks of him in these words, "his look is meagre and emaciated. He appears the wreck of a strong and active man." Immediately before the first bleeding "his pulse was weak and fluttering, so as not to be counted;" but after the abstraction of sixteen ounces "he was free from tremors and pain, and said he felt quite well." "His pulse was now a good pulse;" and although the report states that "he showed some tendency to syncope before the arm was tied up," yet in an hour exactly after the operation "the pulse was eighty and full." This took place on Thursday, 28th December 1826; and on the following Sunday he was bled a second time in the cold fit. The report states, that "the feeling of debility was so overwhelming that he declared he could sit no longer on a chair. His breathing was quick and laborious, and his teeth chattered." The vein was opened, and the report goes on to state, "although a minute before he had declared he could not sit up a moment longer from debility, yet he now said he felt his strength restored, and he had no wish to lie down, and in less than ten minutes I had the pleasure of seeing him running home." His pulse, which before the bleeding was 100, and so weak as scarcely to be counted, when he left the dispensary was 86, and of good strength.

I must refer the reader also to the second case, that of Atkinson, who was bled in the cold stage, which had continued with violent tremors, approaching to convulsions, for twenty-six hours before I saw him. Yet there was no debility either immediately or ultimately. On the third day he was convalescent; he made a good recovery; and had no return of the tremors. He got neither bark nor arsenic. If this man had not been bled it is not easy to say how the case would have terminated; treated according to my plan it could not possibly have ended more favourably.

It has been said that the effect of bleeding in all cases is debilitating. "Even in continued fever, when the disease is cut short by the bleeding, the effect is debilitating; but as in intermittent we may expect a number of paroxysms, the debility is still more to be dreaded in the progress of the disease." Now this is after all only a truism; but allowing that the effect would be productive of thrice the degree of debility, yet who would not joyfully compound to cure the fever at the first onset of the disease even at such an additional expence? The cases I have already published, which were narrowly watched by a multitude, I may say, of observers, proves that this kind of debility is purely hypothetical. Not only in fevers produced by sub-acute inflammatory action of some important organ, but

also in pure inflammations of the same parts, we bleed in order to produce debility, not as a matter of choice, but as choosing the least of two evils. I shall show hereafter that this term, debility, is ever haunting the imagination of the Cullenians. I shall now only mention, that, according to this erroneous system, not only are spasm, delirium, and the tremors, but also the oppression of the pulse, the disorder of respiration, the want of appetite, the nausea, and the vomiting which accompany a paroxysm of intermittent, all ascribed to debility. It is no wonder, therefore, that from *a priori* reasoning the disciples of this system should object to bleeding under such erroneous pathological views. But they should recollect that I bleed only in certain cases of intermittent, not to produce debility, but to restore the balance of the circulation at as small an expence of blood as possible.

3d, Among other objections, one of a very whimsical nature has been made against this practice. It has been said that it cannot be successful, because "it attacks the *effect*, and not the *cause*, of the disease." Let me ask who knows anything of the cause of any disease which affects mankind? There is a great deal of pedantry and ambiguity concerning this term *cause*, as it is generally used in medical language. In employing it, some mean to express the agent, whether known or unknown, which actually induces the disease. For example, the application of boiling water to the surface of the body produces inflammation and its consequences. This is a known cause. An imaginary substance, to which the name of marsh miasm has been applied, is an example of the second. It is this unknown substance to whose agency intermittent fever is ascribed. Another sense in which the term *cause* has been applied is the first diseased action induced by either of the agents. It can easily be shown how very absurd this objection really is, taking it in either sense. In the case of the scald we are called upon to treat the effect and not the cause;—the hot water is removed, but the effect remains.

A. B. has intermittent fever; the cold stage is long and severe; the constitution is too much oppressed, and the patient dies; or rather let me suppose, he would have died but that he is bled. Is it not absurd to object to the practice merely because the practitioner is not "measuring swords" with the cause, viz. the marsh miasm, but treating the effect? Or, A. B. has had the cold fit, and is now labouring under high excitement, and the powers of the constitution are unable to produce the sweating stage; inflammation of the head, chest, or abdomen takes place; or it may prove to be a continued, remittent, or bilious remittent fever; are we not to treat the case

pathologically, because we should only be attacking the *effect*, and not the cause of the disease?

In the other sense it will be seen to be an equally erroneous objection, viz. the first diseased action produced by any agent whatever. Let me ask who can point out the first link in the chain of morbid action. Is it in the nervous system, or in the vascular? Both are seriously involved. If in one, how is it communicated to the other? Who knows the structure of a nerve, and who is acquainted with its physiology? If in the vascular system, whether is the primary diseased action in the arteries or veins, in the capillaries or the trunks, or is the blood itself affected?

It is wholesome to put these questions home to that medical man who is too nice in investigating the occult causes of diseases. If the practice pursued by medical men were to be rejected, and condemned as being unsatisfactory and unscientific, because they treated the *effect* and not the *cause* of the disease, according to either sense, I fear the profession of medicine would soon be at an end, and its professors left in a more miserable plight than Shakespeare's apothecary. Upon this principle, it is unsatisfactory and unscientific pathology which leads us to bleed, blister, and give purgatives for the cure of any acute disorder, because we are treating the effect, and not the cause of the disease.

But perhaps the term "*cause*" is used in the same sense in which Cullen employs the expression "*proximate cause of the disease.*" There never was a phrase invented in medicine which has occasioned so much embarrassment to students. Cullen means it to represent the actual disease itself. If the term "*proximate*" is necessary at all, it should be used to denote the proximate cause of the symptoms. If the objectors to bleeding in the cold stage acted upon this interpretation, I should feel much obliged to them to explain to me what intermittent fever is? I may here tell them I will not take Cullen's definition in answer. Like all, or almost all his definitions, it is erroneous; and moreover, it tells nothing of the "*proximate cause of the disease.*"

4th, Another objection has been made, that bleeding in the cold stage is only, to say the very best of it, a "*palliative remedy.*" The same objection applies to bark and arsenic. "It may, however, be remarked," (says Sir James Fowell at p. 382,) "that, in taking a review of the general practice in intermittent fever, the means usually adopted appear to have had no other view than to lessen the inconvenience of the paroxysms, and that they have not always been sufficiently active to put a stop to the disease in a way that was satisfactory to the patient or

to the practitioner." To say the very worst of it, therefore, the practice does not stand on weaker ground than the other remedies. I have seen much mischief done by the use of bark in aguish districts; and I have known one man killed by arsenic. Clark (On Long Voyages) mentions a similar accident, but I have as yet known nothing but advantage to proceed from bleeding in the cold stage. I wish not to be understood to mean that bark and arsenic will always produce bad consequences, or that bleeding in the cold stage will invariably prove beneficial. I am convinced that both kinds of remedies require sound judgment in their application; and that, if there is any organic engorgement or alteration of structure, bark must be injurious, if it has any effect at all, and that this is one of the cases likely to be benefited by bleeding in the cold stage.

5th, This practice has also been objected to, because it is a *mechanical* remedy. It is said "to affect the powers which move the blood, but it cannot affect the altered state of the blood itself." That it does affect the powers which move the blood is a fact which cannot be denied, and this is precisely one of its great advantages. But to say "that it cannot affect the altered state of the blood" is a mere assertion. I maintain that it does also affect the altered state of the blood. Without entering into the physiological controversy about the nature of the changes which the blood undergoes during its circulation through the lungs, I may content myself with stating the fact, that some change necessary to life does take place on the blood in the lungs. From the commencement of the cold stage the condition of the respiration decidedly proves that the functions of the lungs are much embarrassed. It is not even necessary to inquire into the cause of the pulmonary distress. They cannot perform their functions;—does it not therefore follow that the blood cannot undergo the usual and necessary changes? The blood is in a morbid condition, and when taken from a vein in very severe cases it looks black and does not coagulate. Under such circumstances, when bleeding is had recourse to, it relieves the circulation, unloads the vessels of the lungs, and thereby enables them to perform their functions; the blood is acted upon, and the usual changes are effected. Therefore this "*mechanical remedy*" does also affect the altered state of the blood.

But there is another interpretation of the expression "*altered state of the blood.*" It may relate to a supposed alteration produced on the blood by the morbid agent, the marsh miasm; and I have no doubt this is the sense intended to be conveyed. In the first place, I may remark that we know nothing whatever of this marsh miasm; we assume the existence of

such a substance; and some have even ventured to give it sensible qualities, as smell and specific gravity. This is certainly going quite far enough in mystery and darkness; but to say that it directly affects the blood is a gratuitous assertion well becoming a true Cullenian, whose whole system of physic is founded upon, and carried on from page to page by the most erroneous and the weakest assumptions. This has always been my great objection to this system. Its author lost sight of the true Hippocratic maxim in the investigation of diseases. Cullen declared that there were more false facts in medicine than false theories; and on one occasion he asserted in his lectures that what were called medical facts were nothing more than medical lies. It will be seen that whenever Cullen came to a difficulty, instead of waiting patiently for an accumulation of facts to enable him to investigate all its bearings, he made a leap over the obstacle by assuming a certain thing for a fact. He established a system of special pleading, and a symptomatical pathology, which have been exceedingly injurious to medical investigations; and it will soon be generally acknowledged that his labours have retarded, rather than advanced, the progress of medicine.

It has also been said that "bleeding in the cold stage has generally been condemned, and probably from experience." I beg most respectfully to ask by whom has it been practised, and for what reasons condemned? * In the original paper I have made a general allusion to the cases in which it appears to me this practice is likely to be beneficial, so that I need not again trespass on the patience of my readers.

By bleeding in the cold stage we give our patients the benefit of the following four advantages:—

1st, The injury which in many cases results from the continuance of the venous engorgement, which so constantly leads to organic diseases, is avoided.

2dly, The danger proceeding either from the want of sufficient reaction, or from its excess, is also avoided.

3dly, The practice prevents debility, in a direct manner, by saving the vital fluid.

4thly, The chance of a return of a paroxysm is diminished; or if it should recur, the force of the attack will in general be found to be broken; and in that case a most important point will be gained, by affording an opportunity for the administration of other remedies, as bark or arsenic, which might previously have been exhibited in vain.

* I have shown in a note at p. 277 the manner in which it is probable this prejudice originated; for a prejudice it is, of which those who have so long acted under it have much cause to be ashamed.

Upon the *first* of these advantages I need scarcely insist ; for no one has attended closely to the phenomena accompanying and succeeding severe paroxysms in marshy districts without meeting with mischief produced by the continuance of venous engorgement.* When the sweating stage comes on, it relieves some of the more urgent symptoms, together with the burning heat of the skin ; but all the symptoms are often imperfectly mitigated. Pain in some region continues to distress the patient ; his nights are disturbed ; thirst continues ; the bowels are out of order ; the tongue does not clean ; the appetite does not improve ; and the strength declines. These indications of disease may be slight or severe. If only slight, the intermittent still continues its regular paroxysms in spite of bark and all other remedies, and sooner or later the constitution gives way ;—but if these symptoms be severe from the first attack, the health sooner breaks up, and the usual remedies are found to be equally inefficacious. In either case the constitutional disturbance may be only the effect of functional disorder, proceeding from the continuance of venous engorgement of one or of several organs ; or the same symptoms may be occasioned by structural derangement from the first, generally from sub-acute inflammatory action. In the former case removal to a dry elevated situation, attention to the bowels, and to regimen, will in many instances perform a temporary cure ; whereas in the latter case the same means will have no effect whatever in restoring health, and the usual remedies, bark and arsenic, almost invariably prove injurious. I would also observe that in such cases the sulphate of quinine is productive of no advantage.

Sir James Fellowes, in his Reports already quoted, in allusion to the pathology of the disease, as revealed by dissection, proves “ the existence of disease of an inflammatory nature, ending in considerable vascular excitement, and determination to particular organs, especially of the liver and spleen, and that the derangements in their structure occasioned those relapses or subsequent returns of fever which took place at distant periods, and at lengthened intervals, from the original or primary attack. In fact (says he) we found *that no radical or perfect cure*

* At page 33 of Chisholm's work, last Edit. will be found the following statement, which was contained in a letter from a medical officer high in rank, it would appear, in the army at Walcheren.

“ In the course of six weeks that I have been here I have sent to England nearly 8000 sick, and will send 3000 more. The prevailing disease now is intermittent fever, with its consequences, dropsies, and visceral obstructions,” &c. Sir James Fellowes, in his Reports at p. 333, states that the prevailing diseases amongst the troops who had arrived at Colchester from Holland, were the remitting and intermittent fevers, with dropsies, diarrhoeas, and affections of the chest.

could be obtained until the congestions which had been formed in these important organs were either lessened or removed." And even Cullen also talks of a phlogistic diathesis prevailing in the system and of congestions fixed in the abdominal viscera. (Par. 234.)

Secondly, Medical men who have practised in marshy districts are aware of the existence of the kind of fever which Dr Armstrong has appropriately denominated "congestive fever," in which the animal heat and the heart's action are rather under than above the natural standard. This is not the proper place to pronounce a eulogium on Dr Armstrong's professional character; but I may state, without impropriety, that he deserves great credit for bringing this matter before the profession, and for illustrating it so beautifully. His views have improved the treatment of fevers generally, and this form of fever particularly, more than could have been expected from the talents and exertions of any one individual. This is the form of fever in which brandy, wine, and bark have been so much employed to obviate debility and putridity, without reference to the true pathology of the disease. This is the form of fever which is very frequent in aguish districts, and it is confessedly very fatal; but bleeding in the cold stage, assisted by the application of heat, and even sometimes by the use of brandy, may be successfully employed. Again, when the rigor takes place, whether severe or slight, there is no telling what mischief is to happen—The patient may die in the cold stage; * or the reaction may run high, and may not be succeeded by the sweating stage. In that case the disease is said to be a fever of some particular type, as a continued, remittent, or bilious remittent. Or inflammation may take place in some internal organ. Often, in warm climates have I seen the oldest and the ablest practitioners of the day perplexed and disconcerted by the occurrence of such circumstances in the course of intermittents under the usual treatment. Sir John Pringle states at page 14, "many of the remittents degenerated into continued fevers, and were often fatal; and the intermittents, by being stopped before the proper evacuations were made, or not secured against relapses, changed likewise into continued fevers, or ended in dangerous obstructions of the viscera." Bleeding in the cold stage in a majority of cases, if employed in time, will, I am persuaded, diminish the chance of such evils, by assisting the efforts of nature in removing venous congestion and easing the circulation, while the hot

* It has unfortunately happened sometimes that the disease has been so insidious, that the second paroxysm, after an imperfect intermission, has completely overwhelmed the patient, and put an end to his existence.—Vide Chisholm, p. 51.

and sweating stages will be altogether prevented from taking place.

The *third* advantage is, that bleeding in the cold stage, instead of producing, will directly prevent debility, by saving the vital fluid.

The best maxim in physic is to get rid of diseased action as quickly as possible, as there is no saying what is to follow in the train of consequences. It cannot be denied that the continuance from day to day of such diseased action as occurs in intermittents will produce speedy debility, so that a patient has to suffer all the loss of strength thereby occasioned before the sweating stage comes on. The profuse perspiration which closes the paroxysm is beneficial principally, although not entirely, as a natural mode of depleting the system. The fluid perspired comes at once from the circulating blood, and there can be no doubt exhausts the strength to a considerable degree. In Ward's case the rigor ceased when 8 ounces were abstracted, and by the time 12 ounces were taken he was free from all complaint. In Bullock's case perfect relief took place when he had lost 15 ounces. At the second bleeding, ten days after the first operation, the unpleasant symptoms vanished when 14 ounces were taken. The same evening he was attacked by violent headach and pain in the belly; he was again bled from the same orifice to the amount of 12 ounces, with complete relief. This individual suffered nothing from debility in consequence of all these bleedings. The force of the disease was broken, and he gradually recovered his health without any of the usual remedies. In Smith's case the tremors, and all the other unpleasant symptoms, which were "more than usually distressing," ceased all at once, and at the trifling expence of 12 ounces of blood. This was done on 7th March, and it broke the force of the disease till the 22d, when he had another severe paroxysm. He was not seen by me till the hot stage had taken place, and he subsequently had a severe head attack, which I have no doubt might have been prevented had he been bled again in the cold stage. In the case of Macauley, as well as in that of Clark, the operation was not well performed, owing to the agitation of the body. A small orifice was made, and the blood flowed too slowly. Macauley lost 24 ounces, which occupied fifteen minutes in flowing, and Clark lost 20 ounces, which also flowed very slowly; yet in both individuals the unpleasant symptoms immediately vanished.

In the eighth case the removal of debility by this plan of treatment, in a subject already much reduced by the long continuance of the intermittent, was quite striking. But in none of those cases was there either immediate or ultimate debility. 1

must also refer my readers to the remarkable case of Atkins, the second in my former paper, in proof of the same fact. I could speak of many more cases, and particularly of my own; but I wish to confine my observations to those already recorded, till farther experience shall be obtained on this interesting point. In the meantime, from my own experience, I think I have made out that debility is prevented by this practice, and that one way in which this is produced is by directly saving the vital fluid.

In the *fourth* place, I have to observe, that in some cases there will be no return of the paroxysms, as happened in three of the eight cases published, viz. the first, second, and seventh; and be it remembered they were the worst cases of the whole.* The constitution of the first and seventh had been broken down by repeated attacks, the one in Walcheren, the other in Jamaica. Both had used bark and arsenic, and all the other remedies usually employed in vain. And in the ninth and tenth cases recorded in this communication the good effects are well displayed. In all the other instances it will be observed that the diseases were mitigated. In Bullock's case there was no return of the paroxysms for eight days after the first bleeding; and in the rest, the paroxysms were mitigated in violence, shortened in duration, and attended with much less suffering. As far as my own experience has gone, I am inclined to believe that bleeding in the cold stage will in many intractable cases bring the system into condition to be operated upon by remedies which had previously been exhibited in vain, which must be admitted to be a great advantage gained.

Before bringing this communication to a conclusion, I may briefly mention some precautions which ought to be employed when bleeding is had recourse to in the cold stage of fever. Before a vein is opened the pulse and the temperature of the body should be noticed and recorded. Bottles filled with hot water or heated bricks should be applied to the extremities, and in extreme cases to other parts of the body. A large orifice should be made in the vein, in order to alter the determination of blood at as small an expence of that fluid as possible.

If the cold stage has either continued long and threatens life, or if, after making a good orifice, the blood does not flow freely, it is good practice, I conceive, to give a powerful stimulant, as warm brandy and water. The object of this is too evident to require illustration. I shall only further remark, that, as

* The same result has taken place in several cases since this paper was sent to the press, and in one it has failed. The disease returned at the next period, but the existing paroxysm was cut short, and the man's health improved. These cases will be published in a subsequent Number.

there is a period in every acute disease when bleeding is decidedly injurious, so likewise in intermittent fever there may be a combination of circumstances which might prevent me from carrying it into execution, although I have as yet seen no such case. Care must also be taken to attack any local disease by the appropriate remedies; local bleeding, blistering, attention to the bowels and to regimen.

On the approach of the cold stage the patient covers himself with a load of bed-clothes. If care be not taken by the practitioner to remove them after the bleeding, a hot skin and copious perspiration will likely follow and do mischief, by unnecessarily increasing the debility. In the sixth case it will be observed that considerable heat of skin and thirst took place some hours after the bleeding, which the patient himself "attributed to the pressure of too many bed-clothes, the blankets not having been removed, which he had caused to be heaped upon himself, on the approach of the cold stage."

Great attention should be paid to discover whether or not there is any local disease. It should never be forgotten that some organic lesion must inevitably be the consequence of repeated paroxysms of intermittent. Medical men are too much in the habit of treating disease in a routine manner after they have given it a name, without paying sufficient attention to the pathological condition of the body. In this affection they give bark or arsenic as a matter of course, because it is an ague. There is another error into which they are liable to fall, by imagining that the spleen and biliary system are the parts which exclusively suffer. But affections of the brain, lungs, stomach, and bowels also frequently take place. In fact, there is almost no organ or tissue which does not sometimes suffer in the chain of consequences. I have seen fatal affections of the heart apparently take their origin from intermittent fever; but the lungs of all organs, after the liver and spleen, appear to me to suffer most. The affection is generally bronchitis. It has been long well known that dysenteric symptoms come on with the first paroxysm, and indeed more frequently the dysentery has preceded the intermittent. As long as these internal diseases continue, should they only exist in a mild sabacute form, little benefit can be expected from the usual remedies prescribed in agues.

In concluding this paper I must remark, that several of the cases already published could most certainly have been cured without the bleeding. It was employed, however, to satisfy the scruples of many gentlemen who fancied that there might be some error in my previous observations; and as I knew from experience, that it was at least a harmless remedy, I thought it expedient to show the effect of stopping the rigor,

instantly, as often as I had an opportunity. I had several cases which were treated successfully without either bleeding, bark or arsenic, merely to show my pupils how much might be done where there was no appearance of local disease, by attending to the bowels and to regimen. It has been asserted that the cases I have published are picked samples. If by this is meant that they were cases to which the method of treatment now explained was peculiarly appropriate, it is manifest that the charge is not only too frivolous to require refutation, but really forms the most unequivocal encomium of the remedy. That the cases are not select in any other sense, the testimony of those gentlemen who witnessed them is amply sufficient to prove. The truth is, that I published *all* the cases in which the practice was put into execution before competent witnesses. The only cases not recorded which were treated by me since I have been a lecturer, occurred since my first communication, and shall be laid before the reader in due season.

The object of my next communication will be to show the necessity of abandoning the doctrines of Cullen in teaching the Principles and Practice of Physic.

31, Albany Street, 1st July 1827.

ART. V.—*On the Endemic Fevers of Spain*. By T. PROUDFOOT, M. D. Kendal.—(*Concluded from p. 14.*)

On the Treatment of the Spanish Remittent and Yellow Fever.

YELLOW fever kills sometimes by an affection of the brain, but much more frequently by an affection of the stomach,—an inflammation, which begins in the inner coat, and if unchecked, ultimately involves the whole of the coats, as well as the peritonæum generally. This form of inflammation is widely different from ordinary gastritis, and more nearly allied perhaps to the erythematic species. A local as well as a general treatment is therefore necessary.

To relieve the headach we practise venesection or arteriotomy; apply cold water or pounded ice or blisters to the head; use cathartics and other remedies, according to circumstances, as in severe synocha; but this was found necessary only in a few instances, either when the summer fever was changing its character, or during the hot stage, when there was a remitting tendency, or some other of the peculiarities that have already been pointed out as more particularly affecting the head. Our efforts to relieve the brain, when early and vigorously applied, were generally crowned with success.

But not so those directed to relieve the stomach. To this organ the attention ought to be early and unceasingly directed. Its structure is preserved chiefly by the general remedies,—copious venesection, the warm bath, and mercurial cathartics. When the latter can be brought to operate effectually, and bring away copious and feculent stools, the physician will have the satisfaction to perceive that the tension, fulness, and anguish at stomach are most materially relieved. But it would be wrong, nevertheless, to neglect or delay the use of local remedies. Leeching or cupping, fomentation, and blistering, ought therefore to be had recourse to immediately after the patient is removed from the warm bath; and if the stomach still continues irritable the local means ought to be repeated, and irritating ointments, cataplasms, and reiterated fomentation, can all be used as auxiliaries. Deeply impressed with the necessity of at all times attending to the state of the stomach, and remembering the utility of a solution of sugar of lead in external inflammation, and the sympathy and analogy between the structure of the skin and alimentary canal, I was induced to try the effects of this salt. A grain of sugar of lead dissolved in about two ounces of pure water, to which a little vinegar and a few drops of laudanum were added, was given every three or four hours on the second or third day of the disease, when the irritability of the stomach was excessive, the skin pungent and hot, and the tongue dry, red, and parched; but I could not perceive that any advantage resulted, and it was soon abandoned. The same remedy has often been tried by others, and it is said to have been occasionally found useful; but in this stage of the fever it can only be considered a very secondary remedy. Moreover, it interrupted the use of more certain medicines, particularly purgatives and diaphoretics, the beneficial effects of which, under proper management, were by no means doubtful. Two grains of sulphate of zinc, dissolved in two ounces of water, and exhibited with a similar intent, produced no better effects. In a different stage of the complaint, however, when hemorrhagy took place from the internal surfaces of the body, I have seen the solution of sugar of lead apparently useful when given in the manner stated, or with camphor mixture; but these cases are generally so hopeless that there is little to be expected from any known remedies. An infusion of quassia with sulphate of zinc, or the usual remedies, bark or colomba in infusion, with the addition of a mineral acid, were not more successful. When using bark or colomba I have occasionally tried to move the bowels by a solution of sulphate of magnesia, to which diluted sulphuric acid was added; but the action of the bowels in

such cases was usually almost gone, and blood and matter oozed involuntarily from the anus for some time before life became extinct. Now and then a patient recovered from this very hopeless condition, but the recovery could scarcely be attributed to the efficacy of such remedies.

There are several methods of treating this fever, which have obtained considerable credit amongst those who may be supposed most capable of judging; while others have affirmed, that the mortality is but little affected by any plan of treatment that has yet been devised, and that neither human ingenuity nor patriotic zeal, with their most persevering efforts, have as yet been able to prevent its appearance, or perhaps materially to check its ravages. For the sake of distinction they may be divided into depletory and stimulant, depletory and mercurial, and exclusive mercurial. The method pursued at Carthagena, and what has now been recommended, belonged to the depletory and mercurial rather than to any of the other divisions; but the plan of treatment and remedies were almost infinitely combined and varied according to the supposed exigencies of the case. Opium, camphor, carbonate of ammonia, capsicum, cinchona, wine, and ardent spirits, given as chief remedies at the commencement of the disease, would very generally prove highly injurious; but circumstances often arise during the progress of the fever, when the physician is necessarily induced to make trial of such remedies, which, however, can never be considered in any other light than as auxiliary to others of greater efficacy.

In a high state of febrile action mercury is quite inadmissible, except as a cathartic, and in combination with other medicines of this class. In such a shape experience in this disease is almost universally in its favour. I never used it otherwise than as a purgative, till after copious venesection and the warm bath. The effects of blood-letting, which lessens increased vascular action, and mercury, which produces universal irritability, quick pulse, &c. appear irreconcilable. Whether or not an inquiry into the changes which take place in the destructive inflammation of the stomach would show an analogy between their operations, or how they both become useful, I shall not determine. But, however mercury may act, it seemed to possess very salutary effects when it was preceded by venesection; for no sooner was a salivation produced than the symptoms gradually gave way, the thirst abated, the tongue, before dry, parched, or covered with dark or yellow scales, began to get moist and clean, a pustular or vesicular eruption appeared round the mouth, the anxiety, weight, and uneasiness in the epigastric region began to yield, and the vomiting subsided in cases where, from the violence of the symptoms, there was every

reason to apprehend the occurrence of the black vomit. I only remember two or three instances of recovery after this symptom appeared, and the patients were all strongly under the influence of mercury.

The system is often so torpid that stools cannot be procured by any means whatever, even after the application of external and internal warmth by fomentation and friction with stimulating embrocations and tinctures, and the subsequent application of hot sinapisms to the abdomen, as well as by throwing up warm emollient or stimulant injections, and the combined exhibition of various cathartic medicines. In this hopeless state I have seen mercurial inunction on the abdomen and liver, combined with stimulants internally, and hot mustard cataplasms to the abdomen, apparently rescue patients from the verge of the grave. In cases, too, in which there was diarrhoea accompanied with tormina and tenesmus, a weak quick pulse, and sinking of the powers of life, I have seen a somewhat similar treatment sometimes have the same good effects. Perhaps the friction alone was of utility. But, on the whole, under such circumstances, there is not much to be expected from medical treatment.

During convalescence some benefit seemed occasionally to be derived from light bitter tonics, as an infusion of cinchona, gentian, or colomba, with sulphuric acid, or an infusion of quassia with sulphate of zinc. They were not much used, however, and it was always necessary to guard against their producing uneasiness at stomach. A more eligible plan was, gently to move the bowels by some warm stimulating aperient, such as the pil. rhei comp. pil. aloes cum myrrha, or pil. galbani comp., given in such quantity, with half a grain or a grain of calomel every night, or every other night, as to procure three or four motions on the following day, the interval between the doses being gradually lengthened till good health was permanently established. In cases where a torpor of the hepatic system seemed to have pre-existed, or when the countenance was very sallow, with irregular bowels, half a drachm or a drachm of mercurial ointment was rubbed in every second night on the right hypochondriac region, and sulphate of magnesia, or the sulphate of potass with sulphur, used as cathartics, with great advantage.

There are several circumstances connected with this fever which have given rise to much controversy, which I have intentionally omitted to mention. The great question to be settled is, whether or not yellow fever is contagious. As far as I have seen on the coast of Spain the yellow fever was decidedly not

contagious; but it has always appeared to me to be too dogmatical, and contrary to analogy in other febrile diseases, (as well perhaps as to truth,) to maintain that the disease can never become so under any combination of circumstances.

Innumerable proofs have been given by various writers, which it is quite unnecessary here to detail anew, that the yellow fever has generally, if not always, arisen in Spain from local and atmospheric causes; and consequently, that there it has generally been an endemic disease originating spontaneously, or *de novo*, and has very seldom, or perhaps never, been an imported disease. But analogy in typhus fever might warrant the supposition, that, although the yellow fever of Spain is an endemic disease, originating spontaneously from local and atmospheric causes, and not imported, it may nevertheless be ultimately propagated by contagion in an epidemic atmosphere, under a suitable concurrence of circumstances. Yet I believe it has rarely or never been communicated in this manner; and, at all events, the proportion who appear to take the disease from contagion, when compared with the numbers who take it from local and atmospheric causes, is at all times very inconsiderable. Still, to deny the possibility of transmission by contagion seems to savour too much of that spirit of system, which, the more a physician becomes acquainted with diseases and their causes, generally in a proportionate degree does he feel himself compelled to abandon.

It is now generally admitted that the endemic and sporadic fevers of the West Indies also are not contagious. The diseases usually called autumnal remittent fever, and bilious continued or gastric fever of Spain and the Mediterranean, seem equivalent to these diseases, allowance being made for the severity of symptoms from the difference of latitude; and the transition from these diseases into yellow fever, I believe to be often imperceptible. It has been contended, however, that although a disease, apparently only a different variety of these fevers, may have all the symptoms of yellow fever, still the disease is not genuine yellow fever, but only bears a delusive resemblance to it. The genuine yellow fever, it has been contended, is eminently contagious, and affects persons only once in their lives, like the small-pox or measles. This form of disease, which is held to be alone legitimate in contradistinction to the two former of similar appearance, but of spurious offspring, is supposed to have misled the non-contagionists in their reasonings, and hence to have been the cause of much mischief. But as I set out with a determination to be a mere reporter of what I had seen, and as this great question ought not to be discussed in the

limits assigned for the present paper, I shall rest contented with drawing the attention of the reader to the importance of the question, from the great mortality which this fever has occasioned of late in Spain, and among our troops in some of the West India islands during a period of six years.

At Cadiz in 1800 one-sixth part of the population died in three months. It contains about 140,000 inhabitants. At Xerez de la Frontera, containing about 40,000 inhabitants, 10,000 died of this disease during the same year; and in 1820 the mortality was upwards of seventy in one hundred of those attacked; fortunately, however, the number attacked was not so great as in 1800. At Gibraltar in 1804 the mortality was more than one in three, and only twenty-eight persons escaped an attack out of 9000 inhabitants. At Carthage in 1810 3000 died out of a population of about 12,000, and in the autumn of 1812 about 2000. In the second regiment of foot, quartered at Barbadoes in 1816, out of 390 men seized with the yellow fever ninety died, and of twenty-five officers ten died. In a detachment of the 29th regiment, quartered in St Christopher's in 1812, out of 422 attacked 118 died. Baron Humboldt states, in his personal narrative, that at Porto Caballo the deaths among the Creoles are thirty-three in a hundred; and among Europeans recently arrived the deaths are generally about sixty-five in a hundred.

The deaths among our troops in some of the West India islands during 1796 seem to have amounted to nearly one-half of the whole number employed. Compared with this mortality, the number of deaths now is incredibly small, being only about one in thirty-five of the sick-list. The improvement is partly owing, doubtless, to the very great changes in the medical treatment of disease; but much more is due to the clearing away of brushwood and long grass, and the draining of swamps and marshes—the construction of barracks and hospitals in more healthy situations—and various other improvements for the health and comfort of the soldiers, introduced at the suggestion of the Army Medical Board.

ART. VI.—*Remarks on the Medicinal Properties of Madar, and on the Effects of Bichromate of Potass on the Human Body.* By WILLIAM CUMIN, M. D. Surgeon to the Royal Asylum for Lunatics, and to the Lock-Hospital, Glasgow.

I. **H**AVING observed in the Edinburgh Medical and Surgical Journal, (xxvi. 401,) and afterwards in the Calcutta

Transactions,* an account of a remedy named Madar, which has been long used by the natives of Hindostan in elephantiasis and other cutaneous diseases, I was struck with the similarity of its effects to those of sarsaparilla; and in a particular manner with the smallness of the dose in which it was found to prove most efficient. I determined to make trial of this new acquisition to our Materia Medica, and accordingly applied to Messrs Duncan and Ogilvy, who furnished me with what quantity of the powder I required, with the most obliging readiness.

Madar powder is of a whitish colour; its odour is faint, and somewhat resembles that of opium, and its taste approaches to that of the common horsebean. I administered it to five patients in the Lock-Hospital. The form was that of powder, rubbed up with a few grains of gum arabic; and the dose varied from three to five grains twice or three times daily. In all of the cases it produced on most occasions a degree of nausea, and sometimes vomiting; and these symptoms occurred in some instances very soon after the powder was swallowed, but more generally not till after an interval of an hour or more.

One of the patients was a robust female, who was detained in the hospital by a trivial ulceration, which formed just as her primary sores had been cured. My object here was merely to ascertain the sensible effects of the drug on an individual in a state of good general health. Only slight nausea was produced, and that not uniformly after each dose. The medicine was soon discontinued.

The second case was one of obstinate primary sore, with general derangement of the health, severe headach, and pains throughout the body. The patient stated that she had been under mercurial treatment for some time previous to admission, but without benefit. In this instance the madar was given for seventeen days, at the close of the treatment, after the health had been improved; but the administration of the drug was so combined with various other remedies, that it is impossible to say what exact share it had in the accomplishment of the cure.

A third case was that of a patient of a strumous habit having an eruption of *psoriasis guttata*, slight ulcerations in the throat, and a diffuse tumour over the upper part of the tibia. The madar was given for twelve days without any marked benefit. The eruption yielded afterwards to mercurial frictions, carried to slight ptyalism; and the swelling of the leg, after being punctured, and discharging a quantity of tuberculous

* Paper by Mr Playfair, Vol. i. p. 77.

matter, healed soon after the discontinuance of the mercurial ointment.

In a fourth case the remedy in question was employed for four days only. The symptoms were complicated, but the amendment rapid. This, however, I do not ascribe to the madar in particular, although I have no doubt that it contributed to the result.

The fifth case was very severe and protracted, being an example of secondary syphilis in a highly formidable shape, complicated with pulmonary symptoms. In this instance the beneficial effects of the madar appeared to be clearly shown, when the use of sarsaparilla served only to encourage perspiration, and thus debilitate the patient. As the two last cases possess a degree of interest, independent of the drug which has occasioned these remarks, I shall now introduce an abridged detail of their history and progress.

C. F. a slender female, aged 22, admitted May 23d, 1827. Over the left eyebrow there is a diffuse swelling of the forehead, and on the skin covering it are several small slightly excavated ulcers, florid, with encrusted margins. They arose from small flattened pustules; and two of the same character are still to be seen higher up on the forehead. She has had a copious white discharge from the vagina for three weeks, which was at first accompanied by pain in the hypogastric region. She complains of pain in both legs, increased when in bed; and there is slight diffuse swelling in the shins, immediately below the knees and above the ankles. On the inner side of the left wrist there is also some swelling, but without pain. Four years ago she had sores on the genitals, and subsequently sore throat, which were cured under mercury. Appetite defective; stomach occasionally acid; bowels and catamenia regular; tongue clean; pulse 106.

The usual purgative was given on admission, and next day the small ulcers were directed to be dressed with weak ointment of red precipitate, the legs to be rubbed with tincture of soap and opium. She was also ordered to use an injection of diluted *liquor aluminis compositus*; and to take a drachm of liquor potassæ three times daily, and five grains of madar morning and evening.

28th May.—The ulcerations in the forehead heal; pains and swelling of legs better; complains of inappetence and debility in the morning; madar powders at first caused nausea, but they do not now. In consequence of the state of the stomach I intermitted this medicine, but continued the other remedies, increasing the dose of liquor potassæ to ζ iss. three times daily.

June 4th.—The ulcerations are all healed, one or two being still covered with pale smooth crusts; no discharge for ten days past; tibiæ now free from pain and swelling; appetite and strength improved; to take only $\mathfrak{z}\text{i}$. of liquor potassæ for a dose, and drink daily the decoction of sarsaparilla.

8th.—All her complaints being removed, no swelling remaining in the forehead, and her health restored, she was dismissed cured, with directions to continue the sarsaparilla for a week longer.

In this case the variety of local symptoms present, and the deranged state of the health, certainly led me to fear that the cure would prove tedious, and had mercury been employed this in all probability would have been the result; but by adopting a less energetic mode of treatment, and correcting the acidity of the stomach, the patient was dismissed cured in the short space of sixteen days.

M. M. æt. 18, a slender emaciated female, was admitted January 16th, 1827, with extensive superficial ulceration of the uvula, velum and posterior fauces, cough, copious expectoration of bloody mucus, and severe pain at the upper part of the sternum; bowels and catamenia regular; tongue slightly furred; pulse 112. Affection of throat of a month's duration; pectoral symptoms of fourteen days. No sores or discharge for eighteen months past. By the use of a variety of antiphlogistic remedies, and a blister to the throat, the affection of the chest was greatly relieved, with abatement of pyrexia, and the throat had become less inflamed and cleaner; but the ulceration was not checked, and there was much debility.

January 21st.—The throat was directed to be exposed to the fumes of calomel.

24th.—The tickling cough and difficulty of swallowing having greatly increased, mercurial frictions were commenced; and enemata of beef-tea were ordered daily to support the strength, as little or no food could now be swallowed. A great amendment resulted from full mercurial action, which was soon established; but it was found necessary, from its increasing violence, to stop the calomel fumigations on the 30th, and two days thereafter the frictions also.

February 6th.—The ptyalism was still considerable, but the throat greatly improved. She now complained of severe pains of the upper extremities and shoulders, in consequence of which sarsaparilla was ordered with opium and hyoscyamus at bedtime.

21st.—The pains were somewhat relieved, but the throat was stationary, with return of the difficulty of swallowing, and tenderness of the right *cornu* of the os hyoides. This last

symptom did not yield for some time, until a small caustic issue was formed over the seat of pain.

23d.—No marked amendment of her complaints having taken place, she was now ordered to use, along with the sarsaparilla, small doses of corrosive sublimate.

March 3d.—Painful swelling of the left elbow-joint, and over the ridge of the right tibia near the knee. Blister to the elbow-joint. Leeches to the leg.

20th.—The throat was nearly well, and health improved, but there were considerable perspirations; mouth tender. Omit the corrosive sublimate. To take nitric-acid, with decoction of sarsaparilla.

May 14.—The throat had healed, but returns of ulceration had from time to time occurred.* Pain and swelling had affected the front of both legs, and the extremity of right ulna. Sarsaparilla, arsenic, and anodynes had been given, and leeches applied. The perspirations had continued often to an excessive degree. Last night the pain over the left tibia was so excruciating that I found it necessary to divide the tumefied part nearly to the bone.

17th.—For several mornings past she had spit up some blood, which induced me, as soon as I learned it, to intermit the arsenic, and order a blister for the breast, where she then complained of pain. Although the leg was certainly much easier, her situation was now far from promising, as she still perspired much, and the pulse was rapid. Having made trial of a great variety of medicines, and I may say almost despairing of accomplishing a cure, I now thought of exhibiting the madar, which I did in the dose of three grains twice daily, intermitting the sarsaparilla, and substituting Dover's powder † for laudanum at bed-time.

19th.—Little change, but she complained of nausea in the night. Perspirations have increased. Omit Dover's powder. Take at bed-time laudanum ʒss. with Aromatic sulphuric acid twenty drops. Madar three grains thrice daily.

June 11th.—She had improved considerably, and was now able to walk into the airing ground. The nocturnal nausea and perspirations were still occasionally troublesome, particularly during the preceding night, when she was also much distressed with pain stretching along the front of the left leg from the knee to the toes. She still coughed up in the morning a little fluid, which resembled diluted port wine. Appetite

* A great variety of gargles and other applications had been used for the throat, which it would be tedious to enumerate.

† The Dover's powder has been sometimes found in cases of profuse perspiration to check that secretion.

pretty good. The incision over the tibia had healed, and there was little tumefaction either there or on the ulna. Omit laudanum. Take twenty-five drops aromatic sulphuric acid, with five drops tincture of assafoetida nightly at bed-time. Madar five grains thrice daily. A Belladonna plaster for the knee.

Soon after this a temporary disorder of the stomach caused the madar to be intermitted for a day.

18th.—The tinge of blood in the morning sputum had gradually diminished, and was not observed to-day. There was no complaint of pain in any situation, but the extremity of the ulna was slightly tender when firmly pressed. Her flesh and strength being greatly restored she was dismissed cured.

This case illustrates in a striking manner the value of mercury in arresting the most formidable symptoms of lues venerea, and that too when the disease is complicated with alarming affection of the chest; but in its progress it shows also how groundless is the belief that mercury alone is sufficient for the cure of syphilis. The madar appears in this instance to have had a very salutary effect; to it I confess I ascribe the accomplishment of the cure.

I lately recommended a trial of this drug in a case of secondary phagedænic disease, where there were ulcerations of the throat and nostrils, scabby eruptions, and superficial ulcers. The dose was three grains three times daily. For the first two days the madar caused no unpleasant effect; but it afterwards began to produce nausea and occasionally vomiting, and at length every dose acted like an emetic. After ten days trial of the remedy, the ulceration of the throat having increased alarmingly, it was found necessary to abandon this medicine, and have recourse to a free use of mercury.

Of these five cases in which the madar was exhibited as a remedy, in two it produced no benefit, in two it seemed to contribute to a successful result, and in one I am inclined to ascribe to it the completion of the cure. Being in its nature an alterative, though one of a highly concentrated character, many trials must be made, and a long period must elapse before its virtues can be satisfactorily ascertained. Even sarsaparilla has been conceived by some intelligent men to act merely as so much tepid water; yet it is now admitted almost universally that no unprejudiced person can watch its effects without being fully convinced of its real curative efficacy. Let us therefore not throw aside the madar with neglect, but be encouraged to make trial of this drug, which is productive of no deleterious effects, and promises to prove a valuable acquisition in many unmanageable forms of disease.*

* Our next Number will contain a paper on the same subject, and on the chemical composition and properties of madar, by Dr Duncan Junior.

11. Another remedy of which I have lately made use is the Bichromate of Potass, which was some years ago introduced into the arts as furnishing a beautiful and permanent yellow dye. My attention was first directed to the action of this substance on the human body, by seeing its injurious effects on the hands and arms of some men employed at the Barrowfield works in this neighbourhood. About five years ago I was consulted by two dyers, whose wrists and arms were affected with deep ulcers, caused by immersing them in a solution of this salt. I have since been applied to by a person employed in a manufactory of bichromate of potass, having sloughs on the fingers and *glans penis*, from the application of the salt in substance; but in this instance there appeared to have been a wound or abrasion of the cuticle before the salt caused sloughing. In another instance of a dyer, the solution of bichromate had caused *psoriasis diffusa* of the hands, which, after some time, degenerated into *impetigo*, although during the interval he had been little exposed to the action of the bichromate solution.

The first effect of the habitual application of the solution of bichromate to the skin is to produce an eruption of papulæ. These, after a little time, become pustular, and at length, provided the exposure be continued, deep sloughs form under the pustules. The sloughs are of a peculiarly penetrating character; an instance has even occurred in which a complete perforation through the muscular substance of the hand has in this way been produced. Great differences exist among the individuals subjected to the action of the solution; some suffering very slightly, others severely, so much so, indeed, that swelling of the face and inflammation of the eyes have, I am assured, been produced in addition to the symptoms above described. To avoid the injurious consequences of immersing the hands and arms in the solution of bichromate, an apparatus has been constructed, by which the cloth is passed through the liquid dye without requiring more than the tips of the fingers to be dipped into it. Even this slight exposure, and the handling of the moist cloth after it has been thrown off from the machine, cause the eruption to make its appearance in susceptible individuals; and it would no doubt proceed on through its several stages were they not relieved for a time from the duty. Other liquids used in dying and calico printing produce irritating and injurious effects; thus the solution of chloride of lime causes softening, and sometimes destruction of the nails with excoriation, but nothing resembling the effects of bichromate of potass, the action of which appears to be altogether peculiar.

Soon after I succeeded Dr Brown in the charge of the Lock-Hospital, I began to prescribe a saturated solution of bichromate of potass, as an application to tubercular elevations, excrescences,

and warts. In these cases the new growth has sometimes been removed by absorption without any slough ; but where a slough has formed, it has always served to expedite the cure, and in no instance have I observed it to be followed by a deep or unmanageable ulceration. In a case of warts, more extensive and formidable than any I had before met with or seen delineated, which I treated last winter, the solution of bichromate was the only remedy which acted effectually without causing such intolerable pain as to preclude the continuance of its application.

The bichromate of potass, although decidedly poisonous, does not appear to be very active in its operation. It inflames the stomach, both when introduced into that organ, and when injected in a considerable dose into the veins.* Under these circumstances, it would require some degree of medical hardihood to venture on its employment as an internal remedy ; but it is probable, that, if cautiously used, it may be found, like arsenic, to possess considerable tonic virtues.

Glasgow, 13, Gordon Street,

August 7, 1827.

ART. VII.—*Contributions to Morbid Anatomy*, No. IV. By
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in the University of Edinburgh, &c. &c.

Empyema and Pneumothorax.

THE diagnosis of breast complaints has been in recent times greatly extended, chiefly by the labours of Auenbrugger, Corvisart, and Laennec, although many others, following the example of these able professors, have confirmed most of their observations, added a few, and modified others. In the present communication, however, it is my intention to confine myself almost entirely to the results of my own experience. I have had several opportunities of observing empyema and pneumothorax, both before and since M. Laennec wrote ; and in revising these cases for publication, I have felt an interest in tracing the progress of my own knowledge, which I hope may be deemed pardonable, and even prove instructive to others.

In every case in which air, whatever its chemical constitution may be, is contained between the pleura pulmonalis and pleura costalis, the disease is *Pneumothorax* ; whenever a purulent fluid is in the same situation, the disease is *Empyema*. These affections may occur separately, but are frequently conjoined.

* See Professor Gmelin's Experiments in this Journal, xxvi. 133.

They are recognized during the lifetime of the patients by the common symptoms; or by the more recent methods of investigating pulmonary diseases, by percussion and the stethoscope. It is after their presence has been suggested by the former that we have recourse to the latter for the purpose of acquiring certainty. These never leave the diagnosis doubtful when they are employed; but the unexpected discovery after death of the existence of these diseases in some cases when they were not suspected proves that the common signs are occasionally fallacious. In this country I was one of the first, if not the first, who made use of percussion and the stethoscope as means of diagnosis. Although at first they were employed imperfectly from want of knowledge, I persevered, notwithstanding the ridicule and sneers of the ignorant and prejudiced; because what was said of these methods of acquiring knowledge of internal disease was reasonable, and it was said by men of whose truth and accuracy of observation on other points I was completely convinced. I have now the satisfaction to see that they are duly appreciated by the whole profession, even by those who at first opposed them.

As, in my opinion, pneumothorax is commonly a consequence of empyema, or is preceded by it, I shall first give an account of the cases of the latter which have occurred in my own practice, although some of them were very imperfectly observed, from their occurring when the nature of the disease was not well understood.

Case I.—Simple Empyema of the right side.

Many years ago I had an opportunity of examining a case of empyema, which was remarkable for the enormous quantity of purulent matter contained in the right cavity of the thorax, and for the apparent total destruction of the lung of that side.

A woman somewhat advanced in life presented herself at the Royal Public Dispensary of this place with symptoms which were at the time considered to indicate the presence of hydrothorax. After a few weekly visits she discontinued her attendance. Some months afterwards her husband came to inform me that she was dead, and that she desired him to request me to open her body, as she was sure something extraordinary would be discovered. On inspecting the body as it lay on the table, its external form was not symmetrical, and the right side of the thorax and right hypochondrium were evidently more prominent than the left. My friend Mr Wishart performed the dissection, and when removing the skin and pectoral muscles, a gush of fluid took place, in consequence of the pleura and intercostal muscles being thrust out between the ribs on the right side. The thorax was then opened as quickly as possible, and the right cavity of the

pleura apparently contained nothing but purulent matter. Its quantity was so great as to fill several wash-hand basins, and upon removing it entirely the size of the cavity was truly extraordinary, in consequence of the diaphragm of that side being thrust down into the abdomen, and rendered concave towards the thorax, while the mediastinum was forced over towards the left side. We did not discover any remains of the right lung; and I must confess that at the time I thought it had been entirely destroyed by suppuration. On reflection, however, I soon became satisfied that this could not be the case, otherwise the roots of the large branches of the bronchial tubes must have allowed the purulent matter to escape by the trachea, and prevented the possibility of its enormous accumulation, and fatal hæmorrhagy must have ensued from the trunks of the blood-vessels long before the whole lung could have been consumed. I had therefore no doubt that the lung must have been condensed and compressed towards the mediastinum so as to escape observation. The accuracy of this conclusion was established by future examinations.

CASE II.—*Empyema of the right side.*

The next case I have to narrate occurred in the hospital, and was more accurately observed.

Mark Young, aged 33, a servant, was a patient of the clinical ward of the Royal Infirmary when I took charge of it on the 1st February 1815. He had been a patient of Dr Home, who had considered him as labouring under hydrothorax; but for some time before he came under my care, painful micturition was his principal complaint, and he had few symptoms which could indicate the presence of any thoracic affection. The case on the 1st February was entered in the Journal in the following words: "Complains of severe pain of the urethra, especially about the external orifice, in the lips of which a slight degree of inflammation is observed. The pain is particularly distressing immediately before and after making urine, to which he has very frequent calls. Complains also of difficulty of breathing on going up any ascent, and is subject sometimes to a fluttering in the region of the heart, chiefly a short time after lying down in bed. He feels a slight pain on pressure in the epigastrium, and in the hypogastrium, which last is more severe when the bladder is distended. Pulse 90; heat natural; tongue clean; some thirst; appetite pretty good; bowels have been opened with castor oil; sleep much disturbed by micturition. The micturition and pain of the urethra began about four months ago, which he attributes to his having retained his urine too long. The difficulty of breathing began

about five years ago after an inflammatory affection. Has used since he came into the hospital the warm bath, electuary of sulphur and squill pills, from which last he thinks he has derived some benefit."

He made so little complaint of his pectoral affections, and was so much distressed with the disease of his urinary organs, that my whole attention was directed towards relieving them, but without success. On the 13th of April the report is pretty full, and states that, "when he has been lying quiet he has much less pain in making water than formerly, and voids more at a time; but if he is moving about, or after having a stool, or passing flatus, the pain is increased. For some time he has had some cough and expectoration, which is rather difficult. The acidity of stomach is considerably relieved, but recurs occasionally as well as flatulence. His appetite is entirely gone, and he is greatly emaciated. He sleeps tolerably, and his bowels are costive, having required fifteen pills to open them yesterday. Pulse 146, small, but sharp." He now expressed a wish to leave the hospital. It was found, however, that he was quite unable to bear the fatigue of being removed. He became much distressed with acidity, flatulence, and retching. His urinary complaints were if possible aggravated. He passed a disturbed night, unable to rest in any one position, with a constant desire to make water, but voiding scarcely a drop or two at a time, and he died in the course of next day. The examination of the body was very minute, and proved highly interesting. The dissection was performed, and the appearances described by the late Dr John Gordon, and is interesting on account of the morbid appearances observed in the urinary organs as well as in the lungs.

In the chest.—On removing the sternum the pleura costalis of the *right side* was found much more thick than natural, and apparently possessed of unusual vascularity of its inner surface; the right cavity of the chest contained 130 ounces of fluid, which at first sight seemed to resemble the serum of the blood, only somewhat more opaque, but on its being agitated it very nearly resembled cream, on account of the admixture of a thicker and heavier fluid which had fallen to the bottom.

There were also found adhering to the sides of the cavity, masses of a curdy or flaky matter of different sizes, the largest about the length and half the width of the hand, in contact with the diaphragm.

The lung of this side was pressed by the fluid to the upper and posterior part, lined by the pleura pulmonalis, which, as well as the lung itself, seemed externally quite healthy, excepting in size, being only about one-eighth of what is usual,

or a little larger than the fist. On making a section through it, its internal structure was found nearly natural. At the superior part of the upper lobe, however, an abscess had formed about the size of a large plum, containing purulent matter; it adhered also very firmly to the mediastinum and upper parietes of the chest, and the ramifications of the bronchia were more contorted than usual. On the *left side* the lung behind the cartilage of the ribs was covered with a gelatinous effusion, and adhered firmly to the ribs, mediastinum, and diaphragm; the external appearance was otherwise healthy. On making a section into it, small abscesses were found distributed through every part, and the branches of the bronchia contained a considerable quantity of pus. There had been no effusion into the left cavity of the chest, and the lung was of natural size.

The *pericardium* and heart were quite healthy, except that the cavity of the former contained about ȝiii. of a clear yellow fluid.

Abdomen.—The *liver* was found adhering to the diaphragm, and its external surface was of a colour much paler than common. Its structure appeared healthy in every way, excepting in colour, which resembled the external surface.

The *gall bladder* contained a small quantity of pale yellow bile. The *spleen* was quite natural.

The *stomach* without any mark of disease, as also the *intestines* generally; but a few parts seemed to possess more than usual vascularity, and were somewhat livid. The *colon and rectum* were very contracted, being not so large as the *small intestines*.

The *glands* of the *mesentery* were many of them enlarged to about the size of a small bean.

The left *kidney* and *ureter*, in every respect perfectly free from disease, as was also the cortical substance of the right kidney, and the greater part of the medullary; but the projecting papillæ, called the processus mamillares, were in an ulcerated state, and their membranous coverings, the infundibula, nearly destroyed. The pelvis was enlarged and filled with a granular matter, resembling particles of inspissated pus, mixed with some pus in a fluid state. The ureter at its commencement was found much dilated, being about the diameter of the little finger, and throughout its whole extent three times its usual size.

The inner coat of the *bladder* was universally inflamed, and extremely vascular. In some parts the vessels were observed beautifully branching off from a common trunk in an arborescent form. In many parts this coat was ulcerated, and in some perfectly destroyed. The ulcerations were irregular, and run-

ning into each other, resembling a substance moth-eaten. More were observed in the fore part and fundus than on the back part and neck. The entrance of the left ureter was quite natural, excepting the general inflammation; but that of the right was in an ulcerated and thickened state.

The prostate gland without any obvious disease; as also the urethra so far as it was examined.

This man therefore suffered under two distinct affections, disease of the urinary organs and empyema, and I have little doubt that his death was to be ascribed to the former affection only, and that his emaciation and fever proceeded entirely from irritation and want of sleep. Indeed, it was remarkable how little he suffered from the presence of so great a quantity of fluid in the cavity of the pleura. After he came under my care he had no remarkable dyspnœa. His face had neither a leucophlegmatic paleness nor a bloated flush. His feet did not swell. He could lie on his back or on either side. He did not start from his sleep, and for a long time he made no complaint of palpitation, nor was there any sense of fluctuation in the chest observed. There can, I think, be no doubt that this was a case of purulent secretion from the pleura, which had probably been actively inflamed five years before the death of the patient, and that the termination of the acute disease had taken place by effusion instead of adhesion. Notwithstanding the lung was compressed into a very small proportion of its natural volume, it had not much appearance of condensation, but was generally spongy, and it floated as usual when thrown into water. My lamented friend Mr Schetky made on the spot a painting from the divided lung, which exhibits well the thickening of the pleura and inflammation of the bronchial tubes.

CASE III.—*Empyema of the left side.*

The history was drawn up with great minuteness and accuracy by my promising but now deceased pupil Dr John Lane of Cork; and for some additions to the Journal report of the appearances discovered on dissection, I am indebted to my friend Dr Craigie, who carefully examined the parts after they were removed from the body.

“ November 12, 1820.—Rebecca Neilson, æt. 25, married. Complains of oppression and great difficulty of breathing, of a frequent cough, attended with a free expectoration of a thick purulent-like matter, and of palpitation confined to the epigastrium and that part of the chest lying behind and beneath the *right* mamma. These symptoms are all increased on the slightest motion. She can lie only on the left side, and she cannot take a full inspiration. The right side of the thorax is very sonorous, and by means of the stethoscope the air is distinctly

heard penetrating the air cells. (puerile.) But the left side gives on percussion a dull sound and an obscure sense of fluctuation. The entrance of the air into the lungs on this side is indistinctly heard in all parts, a little more distinctly along the side of the dorsal vertebræ, and above and beneath the clavicle. The action of the heart is faintly heard in the præcordial region, but is very evident in the right side of the sternum. The centre of this action corresponds with the cartilage of the fifth rib. On the upper and inner part of the left breast there is a circumscribed hardness of about two inches in diameter, the centre of which is soft, and gives an emphysematous sensation to the finger. This tumour is unattended with soreness, but pressure excites coughing, and causes to the patient the sensation of a fluid rising in the trachea.

“ Has occasional chills, chiefly after taking cold drink. Has slight œdema and coldness of her feet and hands, and the integuments of the left eye are œdematous; respiration hurried; pulse 108, very small; heat lower in the left axilla than the right; tongue moist; much thirst, and little appetite; bowels open from medicine. Catamenia appeared last about four months ago, and were then preceded by hysteric fits.

“ Reports that about two years ago was affected with cough, which was soon removed by quack remedies. Was afterwards subject to slight palpitation and hurried respiration on taking exercise. In April she received a blow of a chair on the lower part of the left side of the chest, which was followed by inward soreness, heat, and severe pain of that side, which continued till about three weeks ago, when the tumour on the breast was first observed. In June she had occasional chills, dry cough, and the soreness was aggravated. One night in particular it became very severe, and after drinking some rum-punch she felt a sudden snap, as if some part had given way within the chest, and immediately she threw up about a pint of yellow matter, said to be purulent. The pain afterwards abated, and she expectorated for some days a large quantity of similar matter, having an offensive smell. In the month of July she slept in a damp bed, and a rash, which was then present, disappeared. Soon after this the symptoms of her present complaints came on, which have since been gradually increasing, till her admission into the hospital in November under Dr Spens's care. These few last days her pectoral affection has been somewhat easier. About four months ago all objects appeared to her disfigured and larger than natural. This deception lasted for three weeks; and she says that on one evening her child seemed so terrific as to induce her to throw a knife at it. States that about a month ago she perceived a fluctuating sensation in her chest, which was audible to bystanders.

" She has used some medicines before admission, and has repeatedly blistered her side without relief; and under Dr Spens she has used diuretics with slight amendment "

After she came under my care she had cough in the morning, with copious purulent expectoration; difficulty of breathing considerably easier, when lying on the left side, but aggravated when lying on the back, at which time she cannot speak; and when turned on the right side her breathing immediately became laborious, cough was excited, and her face became purple. On the 16th the palpitation was not so distressing, and the pulsation not so high up in the chest. Soon afterwards her expectoration became suppressed, she had colliquative diarrhoea, some slight delirium, and she died on the 24th. The body was examined next day.

In order that the displacement of the heart might be more accurately observed, the right side of the chest was first exposed, by cutting through the cartilages, and then sawing through the ribs of the same side at their lateral angles, and thus removing a portion of the parietes of the right side, while the sternum was left *in situ*. The lungs of this side, which filled the cavity collapsed, and part of the pericardium distended with fluid, was seen situate considerably to the right of the sternum. For a drawing of these parts in their relative position, in this stage of the dissection, I am indebted to Mr Benjamin Bell, Fellow of the Royal College of Surgeons, then my pupil.

The muscles of the left side of the thorax, on which she constantly lay, appeared paler than natural. Underneath, where the emphysematous tumour was felt, there was no loss of bone, but the intercostal muscles and pleura at that part were ulcerated away, making a free communication between the external tumour and the cavity of the chest.

On puncturing the thorax on the left side, between the fifth and sixth ribs, some pus flowed out, but there was no escape of air, nor was any contained in this cavity.

When the sternum and cartilages were completely detached, and the left thoracic cavity was exposed, it presented from nearly the lower margin of the third rib to the eighth or ninth a complete sac of purulent matter, cream-coloured, pretty consistent, and having at first sight the appearance of what is termed an abscess of the lungs. When, however, the purulent matter, amounting to about four pounds, was removed it was shown to be otherwise. Above, corresponding nearly to the third rib, a membranous adhesion, thick and firm, extended from the pulmonary to the costal pleura, and thus formed the upper wall of the purulent cavity. The lower part or boundary of this cavity was formed by a short membranous adhe-

sion, passing from the lower margin of the left lung to the costal pleura near the eighth or ninth rib. Between these limits the pulmonary pleura was covered with a layer of coagulable lymph, irregular on the surface, with fibrous or cordy ridges, and varying in thickness, being in some places as thin as a shilling, in others as thick as a penny-piece. The costal pleura was in like manner covered with an irregular layer of the same kind; and at the back of the lung, near its root, large masses of lymph, some soft and easily torn, others firmer, like cross bands, passed between the pulmonary and costal pleura. The lymph with which the pleura was covered could be detached in many parts, and showed the membrane below, in general not much changed, unless rather more vascular and less transparent than natural. The left lung not above the size of the natural spleen, for the whole of this space was forced towards the mediastinum, with which it was connected by adhesions as hard as cartilage. The anterior mediastinum was forced sensibly to the right side. The alteration of the situation of the heart was again observed. Instead of being found on the left side, lying obliquely between the mediastinum and the cardiac notch of the left lung, it had been not only thrust below the sternum, so as to be nearly straight, but likewise projected considerably into the right side of the chest.

The whole left lung, and particularly the inflamed surface of the pulmonary pleura was examined, for the purpose of knowing whether any passage existed between the cavity and the substance of the lung or bronchial tubes, and after some time spent in inspection none was discovered. Convinced, however, from the symptoms during life, that some such passage must have existed, Dr Craigie traced the whole surface repeatedly, and by removing the lymph by a blunt probe, at length discovered a small orifice, about the size of a goose quill, which extended about $1\frac{1}{4}$ or $1\frac{1}{2}$ inch through the substance of the lung, and terminated distinctly in a pretty large bronchial tube. The reason of the difficulty of discovering this aperture was, that its margins were obliquely compressed against each other, and the opening thus diminished blocked up with a portion of lymph or morbid mucus, and that the surrounding pulmonic tissue was so much compressed as very nearly or altogether to obliterate the passage. When cut open by the scissors its surface was smooth and villous, like that of a common fistulous sore, but without that uneven appearance which distinguishes recent ulceration.

The portion of the left lung above the upper adhesion, though connected to the costal pleura by one or two slips of false membrane, was pretty sound; the remainder was compressed, felt firm, and sunk in water.

In the superior part of the right lung some tubercles were found in a state of suppuration ; otherwise it was natural.

The pericardium contained about two ounces of the usual fluid ; the heart was rather enlarged ; the right auricle and ventricle contained a good deal of black clotted blood, but was in every other respect healthy.

No morbid appearances were discovered in the abdomen.

This woman's thorax was not symmetrical ; the diseased side was contracted or diminished in front, and enlarged behind ; the ribs of the diseased side were more curved backwards, as they arose from the spine, and flatter as they joined the sternum than those of the sound side, while the spine itself was bent a little towards the right or sound side, being the reverse of Laennec's *rétrécissement du côté*, although the want of symmetry was similar. But it must be remembered that Mrs Neilson's left side was distended to the utmost with pus, whereas Laennec's description and figure apply to the state of the parts after the cause of distension has been removed, and the diseased side gets contracted.

According to the appearances discovered on dissection, this was an example of simple empyema of the left side uncomplicated with pneumothorax ; but it may be doubted whether this had always been the case. The woman herself stated, that, about a month before she was admitted, " she perceived a distinct fluctuation in her chest, which was audible to bystanders." Now this audible fluctuation, according to the authority of Laennec, confirmed by the experience of myself and others, as well as by experiment, cannot take place unless both fluid and air are contained in the same cavity. Also in the notes for a clinical lecture which I delivered upon this woman's case before her death, I mention, " I have noticed, by the stethoscope, under the sternum, a singular sound, which I cannot compare to any thing more accurately than to the fall of a heavy drop of water. This was not repeated oftener than once in every two or three respirations. At another time it more resembled the sudden separation of moist surfaces, as in the smacking of the lips." At the time I was not acquainted with the *tintement métallique* of Laennec ; but as I have now no doubt that it was that phenomenon which I attempted to describe, it is an additional proof that air, as well as fluid, was at that time contained in the left thoracic cavity. But if this were really the case, all the air must have subsequently been absorbed or removed. We know by the experiments of Nysten that large quantities of air are readily absorbed by the healthy pleura, but it is doubtful whether the morbid pleura possess that power, and we can easily imagine that the orifice of the bronchial sinus might be

so situate as to permit all the air to be expressed through it, as the thorax got more filled with secreted pus. The description of the circumscribed tumour, between the second and third ribs of the left side, as being emphysematous, and the rushing sound heard in it by the stethoscope, corresponding with the inspirations and expirations, might also be considered as evincing the presence of air in the tumour and chest beneath it, but of this I am very doubtful, for in the last days of her existence the tumour decreased in size, and fell and rose with each inspiration and expiration, and pressure upon it had too instantaneous and direct an effect in producing the sensation of a fluid rising in the windpipe, and coughing caused its protrusion too decidedly to operate through the intervention of an elastic fluid. Besides the sound of rushing might be equally produced by the flowing of a liquid, as of air, backward and forward through a narrow orifice.

The respiratory murmur, being puerile in the right side, was conformable to Laennec's observations; but some doubts may be entertained whether the respiration was really heard in the left side, except immediately under the clavicle, or along the spine. According to Laennec, a very trifling effusion is sufficient to render the respiration inaudible; but although I differ with great diffidence from the authority of one in whose accuracy of observation my confidence increases with my increase of experience, yet I cannot help believing that in other cases of empyema and effusion, besides this, the respiratory murmur of the respirable lung may have been heard, though indistinctly. Nor do I think it improbable that the sound of respiration may be conveyed along the ribs from one side to the other. But after all, there are evident sources of fallacy, and I and others may have been mistaken.

The displacement of the heart observed in this case I have since met with so constantly in empyema of the left side, that it may be considered as a pathognomonic symptom of a considerable degree of this disease.

As a counterpart to the case just related, in which there is reason to believe that empyema, complicated with pneumothorax, had been converted into simple empyema by the removal of the air, is a case which occurred to me before M. Laennec had made us intimately acquainted with the signs of these diseases.

Case IV.—*Simple Pneumothorax of the right side.*

A young man, pressman in a printing-office, consulted me on account of very copious pulmonic hæmorrhagy, which obliged him to discontinue his profession. In the course of some months he again came under my care with symptoms of phthisis pulmonalis in their most advanced stage. He could lie only on the

right side, and was threatened with suffocation if he turned on the left. Suddenly after his expectoration had been unusually copious, he could only lie on the left side, and instead of having the placid tranquillity of a phthisical patient, he became affected with excruciating pains, which he described as if his inside were tearing out, and referred particularly to his lumbar region. Death soon put a period to his agony ; and in this case also the body, before being opened, exhibited some enlargement of the right side ; but on trying it by percussion as it lay on the table, the right side was much more sonorous than the left, which led me to suppose that the right lung was more healthy than the left. On opening the body, however, we were surprised to find the right cavity of the chest of a very large size, and containing no fluid, but distended with air only, which indeed rushed forth as soon as the *pleura costalis* was divided. The diaphragm in this case also was concave towards the thorax, and we here found the right lung compressed against the mediastinum, and bound down by a thickened *pleura pulmonalis*, or false membrane, but we did not discover any fistulous opening through which matter could have escaped into the bronchia. Nevertheless, I have no doubt that this had been a case of empyema, and that, so long as the patient could lie only on his right side, that cavity had been filled with purulent matter, which, in the progress of the disease, had found its way into the bronchia and been discharged from the mouth, while air entering through the same passage into the cavity changed the disease into pneumothorax ; after which the patient could only lie upon the left side, and experienced extreme suffering from sudden distension of the cavity and displacement of various viscera by the thrusting down of the right half of the diaphragm and liver.

The only other case of simple pneumothorax uncomplicated with empyema which I have seen, was that unique instance related by Dr O'Brien in the *Edinburgh Medical Journal*, Vol. xxiii. p. 412, in which it was produced by a rupture of the trachea by external violence.

Most commonly pneumothorax is complicated with empyema, and indeed depends upon its presence. Air may get into the cavity of the chest in various ways, but the two which are most obvious, and of both of which I have met with examples, are the entrance of the atmospheric air through an incision made for giving an exit to the pus in cases of empyema, and its entrance through a bronchial tube, by which a portion of the purulent fluid has escaped.

I shall relate the cases of pneumothorax complicated with empyema in the order in which they occurred, as they thus elu-

cidate the progress made in the diagnosis of the diseases by the extended application of percussion and auscultation.

Case V.—Empyema with Pneumothorax of the Left Side.

For the very minute and accurate history of this case I am also indebted to Dr Lane, who was at that time my clinical assistant. I have been unwilling to abridge it, because these diseases cannot be understood except by studying the symptoms in detail.

November 29th, 1820. John Tough, ætat. 24, sailor. Complaints of cough, which is induced by change of position, especially on lying down and rising up out of bed, or when he sits down to rest after ascending a steep place, or generally on any hasty movement of the body. If he lies down carefully the cough does not come on at all. If he rises up carefully he has no cough in the act of moving, but coughs for about five minutes after being seated. The cough is occasionally followed by vomiting of clear water, which he ascribes to the violence of the cough. It is always accompanied by the expectoration of a clear tenacious fluid, of a salt taste, and so frothy that he compares it with soap suds. The vomiting relieves the cough, as does also the expectoration, which amounts in quantity to about two or three table spoonfuls at a time; the distance of time at which the cough occurs after expectoration being, he thinks, always in proportion to the quantity of fluid expectorated, unless brought on by some sudden change of position. The cough is always relieved by walking in the open air. He can take a full inspiration without hurried breathing; no dyspnoea at present; he lies while awake on his back, to avoid an uneasy sense of fulness in the stomach, which he almost always experiences on turning to the left side; he dozes sometimes on his back, but in order to sleep he turns himself on the left side, when, although the uneasy fulness is felt in the stomach and upper part of the belly, it is not sufficient to prevent him from sleeping; he dreams much, but seldom wakes in a fright, and he feels refreshed by sleep; no sense of suffocation. When he turns on his right side the cough becomes very violent, and he expectorates much larger quantities of the clear fluid before described at one time than he does during a whole day while lying on his left side. It then amounts to half a Scotch gill in five minutes, and pours from his mouth in a continuous stream. The uneasy fulness of the left side disappears when he turns to the right; when he turns to the right side slowly and carefully, and breathes with gentle and uniform respiration, the cough will not come on for ten or fifteen minutes, but if he speaks or breathes with the least quickness, cough is instant-

neously induced, and the beat of the heart becomes stronger. While lying on the right side he feels universally relieved and refreshed until the cough comes on.

He reports that, upon any sudden or considerable movement of the body, he is sensible of a distinct fluctuation in the left cavity of the thorax. When he moves his chest from before backwards, it seems to move alternately against the breast and back-bone, and when from side to side, he feels it chiefly against his shoulders. By making these movements he can at any time render the fluctuation audible to himself and to bystanders. The fluctuation is distinctly felt by the fingers placed in the intercostal spaces, the opposite side of the chest being at the same time struck; it is also felt by the hand pressed against the diaphragm beneath the ensiform cartilage when he takes several short breaths in succession, and is then accompanied by a distinct sense of fluid moving in the chest. The left side of the chest, which is emaciated, is somewhat deformed, and the intercostal spaces enlarged.

The lateral bulging of the ribs is greater at the lower part of the left than of the right side, so that the spine does not appear to be in the centre. The action of the intercostal muscles, which is seen in the back on the right side, is not observable on the left. The movement of the right side of the chest is much more conspicuous than that of the left. On percussion the left side is sonorous above, and gives a dull sound at the lower part. The right side is everywhere sonorous. Respiration is inaudible along the whole posterior and anterior part of the left side of the chest, except very slightly beneath the left clavicle, and under the axilla. It is distinctly heard in all parts of the right lung.

The pulsation of the heart is not felt at all in the præcordium; very indistinctly in the epigastrium and right hypochondrium, but it is felt with great distinctness at the right mamma, conveying there the same kind of sensation that it does when the apex is situated in its proper place. The double beat of the heart is heard at the right mamma, the pulse at the wrist corresponding to the second beat.

When he passes his water, which is accompanied with much flatus, the fulness of the stomach is much relieved. He occasionally, but very seldom, has pain at the pit of the stomach, but he can at any time bring it on by stretching himself in bed. The pain is as if something crushed the painful part, and when he stretches, it is much increased by pressing the pit of the stomach. Some pain in the right hypochondrium, increased on pressure.

There is an abscess on the margin of the anus; Pulse 90, rather weak; heat and respiration natural; tongue foul, with

occasional bad taste; much thirst; skin cool; appetite keen; bowels open; urine scanty, and passed in very small quantities at a time, with some pain.

He reports that a dry cough came on in March last, and was very violent until eight weeks ago, since which it has gradually abated. From the time his cough came on he lost appetite, strength, and flesh continually, and he was confined to his bed in August. Up to that time he felt constantly inactive and dull, with much inclination to sleep. In August he was seized with an acute pain, which extended over the whole of the left side of the breast and to the left shoulder, attended by shiverings that were followed by sweating. There was also pain at the breast-bone, but he does not remember that the coughing increased during the continuance of these pains. They lasted for four or five weeks, and about a fortnight after their disappearance he first heard the fluctuation in his breast as he was kneeling down to tie his shoe, and it continued to become more evident till about a fortnight ago, since which the fluctuation has been less considerable, as if the fluid was decreased. He remembers that his heart was formerly in the left side, but he cannot tell when he first observed its situation changed.

About fourteen weeks ago, (at the time he had the acute pain in his chest,) he recollects to have felt a violent palpitation at the pit of the stomach, which lasted for a week, and since that time he made no observation as to the position of the heart, until he felt it in its present situation. He has tried a variety of medicines without benefit; enjoyed very good health before the commencement of his cough, which he ascribes to being wet through, without changing his clothes.

It is sufficient to make a few extracts from the reports of this case, which are fully detailed in the Journal. On the 18th December it is reported that a large portion of the left side of the chest is sonorous on percussion; respiration is audible for an inch below the nipple in the erect posture, and the sound of agitated fluid is very distinct when he jerks his body. On the 22d, respiration has become quite audible in all parts of the left side of the thorax anteriorly, except at the lowest part, and is very sonorous. On the 28th he had increased palpitation for an hour yesterday, and the pulsation is felt most distinctly an inch below the right mamma; says he has a loud wheezing noise occasionally in the right lung. 31st, Respiration now heard over a great part of the left side of the chest. When he lies upon his back the anterior part of the left side is much more sonorous on percussion than natural, and the posterior part is quite dull; but when he sits up and bends forward the anterior part

becomes quite dull. January 5th, Thinks he coughs more on the left side than formerly. The last report is on the 12th of January. By examination with the stethoscope there is evident pectoriloquism in the upper part of both lungs, and by applying the finger above the clavicle when he is laid upon his back he can, by agitating his body, render fluctuation perceptible to the touch.

He had for some time been anxious to return to his native place, Hull, and being still able to walk about, he took advantage of a vessel going from Leith to that port to return home. Through the good offices of Dr Conolly, then a clinical pupil, I am enabled to complete the history of this interesting case, by the communication of an account of the appearances on dissection, which was performed by Robert Craven Junior, Esq. Vice-President of the Hull Medical and Chirurgical Society.

“ On the evening of 3d April 1821, the day following his death, I inspected the body in the presence of my father and some other medical gentlemen. On opening the thorax the *pleura costalis* was so adherent as scarcely to be separated; the left cavity contained about three quarts of serum, which was taken away, when several ounces, say six or seven, of pus were found at the bottom towards the diaphragm. The lung on that side was reduced to about $\frac{1}{20}$ th its natural size; there was merely a small portion at the upper part of the cavity, on the surface of which the pleura was condensed and very much thickened; it was firmly adherent to the *pleura costalis* by a semicartilaginous structure, to detach which I was obliged to use the scalpel.

“ The *pleura costalis* was also very much thickened and condensed, and had in many places patches of coagulable lymph, which had become organized. In the right cavity there was no fluid, but the lung was beset with tubercles, and had partial adhesions, which appeared to be recent.

“ The heart was small, but without any evidence of disease; it was displaced and lay under, and somewhat to the right of the sternum; no fluid in the pericardium.

“ The abdominal viscera were also examined; the mesenteric glands were considerably enlarged; and there were partial ulcerations of the mucous coat of the jejunum and ileum.”

The next example which occurred to me was that of Mrs Craig, which I have already published in the Medico-Chirurgical Transactions of Edinburgh, Vol. i. p. 455. The case was remarkable on account of the air not being confined within the cavity of the pleura, but forming on the chest several distinct external emphysematous tumours, illustrative of the tumour whose nature was obscure in Mrs Neilson; and on account of its pro-

gress being the reverse of that which is the most common. The disease having commenced in the parietes of the chest, the pus eroded the *pleura costalis*, entered the cavity, excited chronic pleuritis, and at last found an exit through the lungs by a bronchial tube, establishing a communication between the aerial passages and the external tumours.

Case VI.—*Empyema of the left side terminating favourably after operation.*

Mary Mulrean, aged 8, was admitted into Queensberry Hospital, September 1822, with severe pulmonic inflammation, for which she was bled and blistered, but was removed from the hospital not much relieved. Her pectoral affection became much worse, and was attended with most acute pain. After some time I was consulted. She was now very much distressed, and could breathe only when lying on the left side. The left hypochondrium was generally enlarged, and there was a circumscribed tumour about the size of an egg projecting between the second and third rib in a line with the nipple. This tumour was punctured by Mr Cumming, who then attended her. At the time of the puncture only the pus external to the ribs was discharged, but when laid in bed a copious discharge took place, and when she coughed, it flowed from the orifice in a large stream. A phlegmonic tumour afterwards appeared on the same side at the ninth rib, which was opened by Dr Duffin. This did not discharge freely like the former, but occasionally closed and opened again, and sometimes both orifices were discharging at once. She now became hectic, and her recovery was despaired of. Notwithstanding the abundance of the external discharge she was often like to suffocate from internal oppression. The pus at last seemed to make its way into a bronchial tube. She spat up whole cupfuls of thick pus at a time; and after this she rapidly recovered her health, and is now a stout young woman. For a long time after her recovery, there was a considerable depression of the upper and anterior part of the left side, and the thorax was not symmetrical; the mesial line of the sternum inclined to the right, and the spine to the left. Now the chest is equally elevated on both sides, and there is little deficiency of symmetry.

Case VII.—*Empyema converted into Pneumothorax by operation.*

Mr N. a student of medicine, was seized, after much exposure to cold in the dissecting-room, in the end of 1824, with what was considered as pleurodyne of the left side. When I first saw him he was bedfast, and complained only of pain in

the left side and shoulder, without cough or expectoration. He had also hectic fever. The first symptom which indicated the true nature of the disease was the total immobility of the left side during respiration, and the displacement of the heart, the two auricles of which could be distinctly seen pulsating on each side of the upper part of the sternum. The diagnosis was confirmed by the obscurity of sound elicited by percussion, and the absence of the respiratory murmur in the lower portion of the left side. After consultation with my friend Dr Abercrombie, it was resolved to perform the operation of *paracentesis thoracis*, and to complete the removal of the pus by means of a syringe. The operation was performed by Dr Ballingall, and was attended with some difficulty, on account of the very narrow space between the ribs. The quantity of pus which issued spontaneously was considerable, and much was drawn off by the syringe, in all three or four pounds. The syringe was afterwards repeatedly employed at intervals. He derived very great relief from the operation; but notwithstanding every precaution, as the lung did rise to occupy the space of the pus, air entered by the orifice, and converted the case from empyema into pneumothorax. The diseased side now became partially morbidly resonant on percussion, and the *tintement métallique* was distinctly audible. The wound continued to discharge for some time, but at length closed. His breathing having then become more difficult, another opening was made some weeks after the first, when several pounds were again drawn off with considerable relief. After long suffering he died exhausted and hectic, about ten months after the first operation, and the only symptom worthy of notice was that, towards the close, the muscles on the right side of the neck acted most powerfully during inspiration, in the manner pointed out by Magendie.

The body was examined in the presence of Dr Abercrombie and many other professional gentlemen, and I am indebted to Mr Watson, Fellow of the Royal College of Surgeons, for a sketch of the appearances when the thorax was opened. The sternum was left *in situ*, and the ribs on each side laid back by dividing their sternal cartilages, &c. and breaking them across about their middle. The pericardium, of large size, was seen projecting nearly equally to both sides of the sternum, the upper portion to its right, and the lower to its left. On this side it was covered and bound down by a dense adventitious membrane, which had prevented its resuming its natural situation, when the pus was partially removed. A similar membrane covered the pleura of the diaphragm and ribs. Rather more respirable lung than is usual in such cases, consisting of the upper lobe, appeared at the upper part of the left side, where its

adhesions were general. The lower lobe was bound down upon the mediastinum, and by far the larger portion of the cavity of the left pleura was filled with air and purulent matter, of which a considerable quantity still remained. The lungs of both sides when cut into were perfectly healthy. No bronchial communication was suspected, as the patient had never expectorated pus.

Case VIII.—*Pneumothorax with Empyema of the left side.*

The following case is remarkable on account of the short time in which it proved fatal, and on account of its true nature not having been discovered until a few days before death, although the severity of the symptoms made the patient an object of unremitting attention.

Bell Hood, æt. 25, admitted into the hospital 13th Dec. 1826. Complains of acute pain in the right hypochondriac region, under the false ribs, shooting backwards, with exquisite tenderness on the slightest pressure; some cough, readily excited by motion and full inspiration; which, as well as the erect posture, greatly aggravate the pain of side; expectoration scanty and rather difficult; some pain of throat, with redness of fauces, and considerable hoarseness; respiration short and frequent; countenance anxious and flushed; occasional fits of vomiting, which are much excited by drinking cold liquids; tongue foul and moist; pulse 130, full and rather sharp; state of surface of the skin variable; heat moderate; bowels very confined.

States that six weeks ago she was first affected with a slight degree of pain in the right hypochondrium, which has been gradually increasing in severity; sore throat supervened a fortnight ago; has had frequent severe rigors; has not been bled; and has used no medicine.

14th.—Twenty-two ounces of very buffy blood were yesterday evening drawn from a vein; this produced a feeling of faintness, with relief to the pain, which, however, returned in two hours. Twenty-four leeches were then applied to the side, and the pain is now referred to the epigastrium, with tenderness of the scrobiculus cordis; respiration short and frequent; and she moves both the ribs and abdominal muscles; respiratory murmur heard with difficulty on the right side, and is puerile in the left side; lies only on the left side; has short suppressed cough, with little expectoration; the sputa are colourless, clear, and tenacious; pulse very frequent, 130 and upwards, and difficultly reckoned; tongue clean; bowels open.

Twenty-four leeches were directed to be applied to the epi-

gastrium, and afterwards cataplasms; the opiate linctus to be taken when the cough was troublesome.

15th.—Leeches gave great relief; had three scanty lumpy dark-coloured stools from a compound jalap powder, with some tenesmus; had slight vomiting during the night, but slept well without return of pain until about five hours ago, and it is still very slight without tenderness; countenance and respiration improved; pulse 134, firmer, and soft; tongue nearly natural; great thirst; urine high-coloured.

A purgative clyster was ordered; the linctus continued; and acid drink at pleasure.

16th.—The injection operated twice, and produced dark lumpy stools; became delirious during the afternoon; got a camphor draught, which composed her until about 2 A. M., since which time she has walked the ward and changed her bed from uneasiness and restlessness, but she asserts that she was all the while quite conscious. She now complains of great pain in the right hypochondrium and right side of the chest; respirations are 48 in the minute; slight cough; pulse 144, very weak and irregular; tongue clean.

She was ordered to take the camphor draught immediately, and again at 9 P. M. and at 3 A. M., if necessary. A purgative clyster was to be immediately administered, and tartar emetic ointment to be rubbed on the side.

17th.—At eight o'clock yesterday evening the difficulty of breathing and general distress were greatly increased, with excessive irritability, and the pulse about 160. A turpentine clyster was administered, and an anodyne draught given, after which she became more composed, and her breathing easier; a very scanty evacuation followed the clyster.

During the night she is reported to have talked incoherently, but her mind is now quite correct; respirations 48 and laborious; larger on the left than on the right side of the chest. When she lies on the back the right side is more sonorous on percussion than on the left. When the stethoscope is applied to the right side no respiratory murmur is heard, but in the left it is very audible and puerile. When sitting up and the naked ear applied to the chest, mucous rattle is heard in the left side, and occasionally in the right, alternating, if I mistake not, with *tintement métallique*, chiefly when she first sits up, but not rendered more certain by coughing or speech. Cough more troublesome, and there is very audible mucous rattle. Expectoration difficult from inability to get up the sputa, although she has strength to sit up in bed.

She was ordered to inhale steam, to take small doses of ipecacuanha frequently, and to continue the tartar emetic frictions.

18th.—Continued sensible for about an hour after the visit, when she became delirious, and continued so until 5 P. M. She then got out of bed, walked a few paces across the ward, but was obliged to be carried back from sudden weakness of the lower extremities, and she died almost imperceptibly soon after she was laid in bed.

The body was examined next day. On percussion, the right side of the chest was much more sonorous than the left, but no difference in their size or form was perceptible. The body was very fat. When the abdominal parietes were turned back, the liver came fully into view, enlarged and thrust down below the lower edge of the ribs. There was no effusion, inflammation, or other disease of the abdominal viscera. From the abdomen the left half of the diaphragm was felt to have its usual form; but the right half was convex downwards, and to the touch was elastic, and seemed to confine air in the chest.

The left side of the thorax was first opened; the lung collapsed, and nothing preternatural was observed; but on prosecuting the examination a rush of air, without fœtor, took place from the right side of the thorax; and on removing the sternum, the right cavity, greatly enlarged by the descent of the diaphragm, was nearly empty, containing only twenty-two ounces of sero-purulent fluid, and apparently no lung. The whole cavity was lined with coagulable lymph, of a primrose colour, and varying very much in thickness. It was thinnest on the lateral costal pleura, but became thicker as it approached the spine and diaphragm, and it covered what appeared to be the mediastinum with a thick layer, from which massive bands stretched across the cavity to the diaphragm and lower portion of the costal pleura. It was of a soft consistence, and in no degree organized or penetrated by blood-vessels from the contiguous pleura, which was in general highly vascular.

The left lung and heart were now removed, the trachea cut across, and the mediastinum with all that adhered to it, and a portion of the right diaphragm, carefully dissected out. It was now quite evident that the right lung was concealed behind the thick layer of coagulable lymph, which seemed to cover the mediastinum, but bound the lung down upon it. The lung was compressed into a flat mass with thin edges, about six inches long and three and a half broad, and about one or one and a half thick. Its lobes were glued together, as was the mediastinal surface, slightly to the mediastinum, but without any interposed lymph. The substance of the lung felt doughy, but did not retain the impression of the finger. To ascertain whether its surface was entire the lung was wholly immersed in water, and air was blown into the trachea after the left bronchial branch was secured, upon

which bubbles escaped readily through the coagulable lymph from a single orifice on the lateral surface of the lung, and about an inch from its dorsal edge. This orifice was about one-eighth of an inch in diameter, and led into a very small superficial cavity, not lined by any membrane, or communicating with a visible bronchial tube, but as if formed by the breaking down of a small portion of the lung, laying open a number of air-cells through which air could be blown from the trachea. The lung itself was somewhat condensed, but swam in water, and could be inflated in a very slight degree only. On cutting into it no great deviation from the healthy texture was observed, and it contained no tubercles.

The left lung, except a few old adhesions, was perfectly healthy, and the heart was natural in size, structure, and the distension of its cavities. Fibrinous polypi, very firmly attached to both ventricles, stretched into the aorta and pulmonary artery.

Very little blood escaped during the dissection.

Case IX.—*Pneumothorax and Empyema of the right side with partial adhesion.*

Elizabeth Reid, aged 37, was admitted into the clinical ward on 22d February 1827, under the care of my colleague Dr Graham.

Countenance rather sallow and depressed; complains of general debility and languor, with sense of heat; feels some pain in the course of the œsophagus downwards to the middle of the sternum, coming on every day about twelve, and continuing till the following morning, preventing sleep, and with the general pains around her body, rendering the same position irksome and uneasy when continued in for more than ten minutes. There is some hardness in the epigastrium and right hypochondriac regions; and in the umbilical region a slightly elevated tumour is perceptible, touching which affects the respiration. Some cough, with scanty tough expectoration, and giving slight pain under the sternum.

Palpitations occur every night for about four or five hours, especially when lying on the left side. Anorexia great; the bowels slow; the last two stools observed to contain blood; tongue clean; pulse 116, small, weak, occasionally intermitting; urine very high-coloured, and at times difficulty in voiding it; has got no sleep for the last fortnight.

Says, that for the last twelve years she has been much troubled with a complaint in her stomach, having two or three times a-week for that period had vomiting of greenish yellow matter after any unusual exertion, each attack lasting about

twenty-four hours, but had no pain except in her right shoulder (which she attributed to a strain got about a month since) till twelve days ago, when this pain was much increased; and she was also attacked with severe rigors, general pains, and soreness, with great thirst, and vomiting of bile, for which she was bled the same night to about (she says) three quarts, or thirteen cups, with instant relief. A blister was afterwards applied to the right hypochondrium, not because the pain was more severe there than elsewhere, but because it was said by the practitioner who attended her to be the seat of the disease. The feeling of burning in the chest came on about a week ago, the other symptoms having abated, and debility only remaining.

On the 15th March it is noticed that the ribs on the right side of the chest were less movable in inspiration than those on the left, and the sound on the right by percussion dull. At this time the cough was very troublesome, and the expectoration much increased. Coughing also gave a sensation of rawness within the chest.

On the 16th the impulse of the heart was distinctly perceived by the stethoscope on the right side of the chest below the mamma, and from that point upwards in a line to the axilla; the respiration dull and indistinct near to the clavicle, and in most parts of the right side behind inaudible; dyspnœa and cough are increased when lying on the right side; lies pretty easily on her back; tongue florid, with a few aphthæ. Next day the cough is described as more severe, and the expectoration much more copious, yellow, partly flocculent, and partly diffusible through water; complains much of shooting pain below the right axilla, increased by lying on the right side, and going off occasionally when turning to the left.

On the 18th it is reported that during the preceding night she had been able to lie for about an hour on the *right* side without pain or cough, which she had not been able to do since she was taken ill. For some days after this her expectoration continued to be copious and then abated.

I took charge of the female clinical ward in the beginning of May 1827. The nature of the disease was so completely ascertained, and the patient so infirm, that it was not thought necessary to subject her to the distress of renewed and repeated minute examinations. At that time she was very greatly emaciated; had hectic fever; mouth and tongue aphthous, with frequent diarrhœa; very great dyspnœa, most comfortable in the erect position, and able to lie down only on the *right* side, any other position causing pain at the inferior part of the left side; started much during her sleep, but was no longer sen-

sible of fluctuation in her chest ; full inspiration prevented by causing pain in the right hypochondrium ; cough generally slight, and sputum sometimes offensive ; had often a sense of constriction along the anterior edge of the diaphragm, occasionally of something rising in her chest, and after deglutition felt as if an obstacle prevented the descent of the food into her stomach. The right side was generally dull on percussion, and the respiratory murmur was inaudible at its lower part ; the left side of the chest seemed preternaturally sonorous on percussion.

In this hopeless case the treatment was chiefly directed to mitigate painful sensations, and to check the diarrhoea, which was rapidly reducing the strength of the patient.

She gradually sunk, and expired on the 10th June. The body was examined next day with great attention by Dr Cullen, one of the surgeons of the hospital, and I had the assistance of my colleagues, Dr Graham and Dr Alison, in remarking the morbid changes, which were noted down at the time by Dr William Fleming, to whose division of the clinical ward the patient belonged.

Externally the right side of the chest appeared larger than the left, and the sixth, seventh, and eighth ribs of the latter were rather depressed. The superior part of the left side corresponding to the second rib was more sonorous than the same part of the right side, but inferiorly the right side gave a clearer sound on percussion. The ribs were everywhere more prominent than the intercostal spaces. On opening the abdomen no protrusion of the diaphragm downwards was perceived. The cartilages of the ribs were exceedingly strong. There was no air in the cavity of the left pleura. Its lung was lighter in colour than usual, and collapsed upon the admission of air. There were strong adhesions at the upper lobe of this lung, and in the superior part a tubercle, with a small cavity corresponding to the point where the adhesion was strongest. The remaining portion of the lung was healthy, and not tuberculous. The pericardium was in its natural situation. The bronchial glands were slightly melanosed.

Upon making a puncture between the first and second rib of the right side, no air escaped, nor did the lung collapse, being prevented by the strong adhesions of its whole upper portion to the mediastinum and ribs. Upon puncturing the inferior part of the same side some air escaped, and a considerable cavity, extending from the diaphragm almost to the second rib, was discovered, containing an effusion of sero-purulent fluid, the lung being forced upwards, and compressed by the accumulation of pus, of which there was about two pounds. It exceedingly resembled the pus of a common abscess, containing no

flakes of lymph. Upon blowing into the bronchial tubes some air appeared to escape from that part of the compressed lung, which corresponded to the intercostal space between the second and third rib. The right lung was spongy, crepitous, and less condensed than might have been expected from its diminished size. It was not tubercular. It floated upon water even with its false membrane attached. The pleura of the right side, especially anterior to the angles of the false ribs, appeared very unhealthy, as if the effused lymph had been organized, and not terminating in simple adhesion, but becoming granular, had ulcerated, assuming the appearance of a common ulcer on the leg after death, *viz.* fleshy, vascular, and irregular. The false membrane of organized lymph was evidently vascular, was external to the pleura of the lung, easily separable, leaving the pleura of healthy appearance. The ulcerations as above described on the right side communicated freely with each other, forming sinuses, and had destroyed the continuity of the pleura, but the bone opposed was not carious.

There are some peculiarities in this case deserving of notice. It is by no means certain when the pleuritis, which gave rise to so great organic changes, commenced; or more specifically, whether the false membrane had begun to be formed twelve years or twelve days, or rather a month before admission, when she had pain in the right shoulder attributed to a sprain. The history of the symptoms is in favour of the latter supposition. It was then that she first experienced severe rigors, general pains, and soreness, and other decided symptoms of acute inflammatory affection. During the longer period, the stomach and liver seemed to be the only organs implicated. But, on the other hand, the thickness and cartilaginous hardness of the false membrane, and its being apparently ulcerated as a secondary disease, are scarcely compatible with the idea of its being the consequence of an inflammation which took place only four months before death. The manner in which the paroxysms of vomiting were excited by unusual exertion rather suggested the idea of an organic affection than of simply diseased function; and we can conceive that the functions of the stomach might be deranged by sympathy, when by unusual exertion a diseased lung was called into excessive action, while the absence of all pulmonic symptoms has been repeatedly observed in cases where false membranes have been found on the pleura. It is not improbable, then, that pleuritis had existed in a chronic state for twelve years, and was called into activity by the sprain, as it had all along been exasperated by unusual exertion.

Another curious circumstance was the daily recurring sense

of heat in the course of the œsophagus, noticed at the time of her admission, and which from time to time was the symptom most complained of to the termination of her life. I regret that the morbid state of the pleura should have exclusively engaged my attention, and that I made no observations, or at least preserved no notes of the state of the œsophagus and stomach, to which so many symptoms in the course of her illness were referred. I notice this to put others on their guard against such negligence.

Until the 18th March she could not lie at all on the right, the diseased, side without pain or cough being induced. When she became my patient on the 1st of February she could lie only on the right side. At the time when this change took place her expectoration, till then scanty, became more copious. Now, if we suppose that at this time the effused fluid first found its way into a bronchial tube, the disease was then converted from empyema into pneumothorax, but the change of decubitus is exactly the reverse of what it ought to have been; for in empyema the patients commonly lie on the affected, and in pneumothorax on the sound side. It may, however, be remarked, that the reason why she could not lie on the sound or left side during the latter period of her disease was not increased difficulty of respiration, but pain induced in the left side from that position.

This case also illustrates the modifications of the signs of percussion and respiration, in consequence of adhesions.

Case X.—Pneumothorax and Empyema of the left side.

The last of this series of cases occurred soon after the preceding.

Margaret MacCromby, æt. 32, admitted July 13, 1827. Has very intense dyspnœa, obliging her to lie with the head much raised; totally preventing decubitus on either side, and frequently amounting to complete orthopnœa; the respirations are much accelerated; at present 50 in the minute; the left ribs are little if at all elevated during inspiration; she has some cough, with scanty, very difficult expectoration, often causing retching; experiences much pain in the posterior part of the left side of the chest, extending over the shoulder down the arm to the elbow. There is increased heat of the integuments of these parts, with tenderness on pressure; has much palpitation; perspires very profusely during the night; and has severe diarrhœa; pulse 136, sharp; great thirst.

The sound on percussion is very obscure over the whole left side of the chest, as well anteriorly as posteriorly, except beneath the clavicle, for two or three inches, where it is preterna-

naturally clear and sonorous. On the right side it is natural ; no respiratory murmur can be perceived in any part of the left side, excepting for a small space between the spine and scapula, where it is "bronchial." The "*tintement métallique*" is distinctly audible on sudden change of position, but not during coughing, respiration, or speaking ; and on succussion of the trunk, the sound of fluctuation is evident. On the right side the murmur is distinct in the superior lobe, and obscure in the postero-inferior portion. Below the right clavicle the sound of the voice is loud, and seems almost to pass through the tube of the stethoscope (bronchophony ?) The heart is much displaced ; it pulsates most strongly near the right mamma, and between it and the sternum. On admeasurement very little difference can be discovered in the capacity of the two sides of the chest ; nor are the left intercostal spaces at all prominent.

Fifteen months ago, after a tedious and severe labour, she was delivered of a still-born child (a leg presentation.) One month after she had considerable hæmoptysis, with much cough ; relieved by venesection. During the last summer she continued delicate, but free from any pectoral complaints. In November last she had again slight hæmoptysis. In the winter the cough and pectoral complaints returned, being exacerbated by any casual exposure to cold. She had but little dyspnœa, and not much expectoration. Six weeks ago, after well-marked rigors, was suddenly seized with acute pain in the left side of the chest, urgent dyspnœa, and increase of the cough. She had also pain and tenderness of the back, left shoulder, and arm. These symptoms have all been rapidly increasing in severity ; and she has at times expectorated large mouthfuls of purulent matter. But during the last three weeks the expectoration has been rather scanty ; the diarrhœa and perspiration have supervened, with emaciation and general debility.

After admission she continued to get worse, and died on the 18th of July.

The body was examined next day with very great skill and attention by Dr Cullen, in the presence of Dr Alison, Dr Dalmas, many of the hospital pupils, and myself, and the appearances were carefully noted by Dr William Henry, to whom I am also indebted for the very accurate history of the case.

About 3ii. of bloody serum were effused into the peritoneal cavity. The left side of the diaphragm formed an arch of great convexity, projecting into the abdomen, and forcing downwards the stomach and contiguous viscera.

The right side of the thorax was first opened. There were firm and pretty general adhesions between the costal and pulmonary pleuræ. The whole of the pericardium and its con-

tents were found on the right side of the median line. The apex of the heart was situated beneath the cartilages of the fifth or sixth ribs, near their union with the bone. The left pleura was carefully separated from its attachments to the cartilages and intercostal muscles, which were dissected back for a considerable extent, when it appeared as a large sac, elastic, and evidently distended with air, projecting far forwards and into the right side. A flexible tube was introduced, and a portion of air collected over water for analysis. About 1b viii. of foetid pus were then drawn off. Two fistulous communications between the bronchia and the cavity of the pleura having been previously detected by insufflation through the trachea, large and numerous bullæ of air ascended through the fluid. The lung was firmly bound down to the spine by an universal covering of dense lymph. Its tissue was much condensed, being no longer crepitous, and sinking readily in water. The lower fistulous aperture was traced obliquely under the adventitious membrane and pleura into a dilated bronchial tube, of which it was evidently the termination.

At the most depending part of the anterior portion of this lung, the false membrane by which it was invested presented several depressions of irregular form, with indented margins, apparently the product of ulceration. Some of these were bounded by the pleura, but others had pierced this membrane, and had penetrated to various depths the pulmonary tissue, which was soft and pultaceous, with very fetid odour, resembling incipient gangrenous degeneration. The costal pleura, with its new membrane, also exhibited a similar diseased condition, chiefly between the angles of the ribs and the spine. In this situation were very many irregular depressions, giving to the surface somewhat of a reticulated appearance. At several of these points the pleura itself was penetrated, and the tendinous aponeurosis of the intercostal muscles exposed; but the ribs were no where denuded. The colour of those muscles was in some parts changed, and they appeared in a state of atrophy.

There was considerable serous engorgement of the right lung, which was soft and pulpy. There were no tubercular depositions in either lung.

Slight increased vascularity, but no ulceration of the mucous membrane of the small intestines.

The air obtained from the cavity of the chest was examined in the evening by Dr Henry and myself.

The volume of the mixed gases had been notably diminished by remaining some hours in contact with water. The presence of sulphuretted hydrogen in quantity was indicated by the blackening of carbonate of lead. One hundred parts wash-

ed with liquid potassa lost twenty-six parts, which were regarded as a mixture of carbonic acid and sulphuretted hydrogen. On the addition to the residuary gas of twenty-six measures of nitric oxide, no fumes or diminution of volume ensued. It was therefore inferred to be pure nitrogen.

In these two last cases, as well as in some of the others, though not so accurately observed, the false membrane had the appearance described by Laennec, as arising most commonly from hemorrhagic pleurisy, "*une dureté particulière, une demi-transparence bleuâtre, et une commencement d'organisation fibreuse, ou analogue, à la souplesse près, à celles des cartilages,*" T. ii. p. 121. In none of these cases, however, which I observed, did I see any proof or reason for believing that the fluid originally effused was bloody. Indeed, I have always considered the bloody effusion, which I have often met with in various cavities, not as the effect of an inflammatory action, but as an extravasation in the agonies of death. I the more readily distrust M. Laennec's opinion, as it is connected with an hypothesis; viz. that the effusions which form the ordinary adhesions after acute pleurisy, and are convertible into serous membranes, are albuminous; and that those which form cartilage are derived from the fibrine of the blood. But he has not supported this opinion by any direct chemical experiments proving the diversity of the nature of the effusions.

I think it highly probable that the false membrane, after it is formed, becomes organized, and is converted into a secreting surface, for in general it covers the whole of the pleura in the cavity where it exists, so completely, that the false membrane seems the only source of the sero-purulent fluid, with which it is gradually enormously distended.

It is also evidently susceptible of inflammation, at least of ulceration; for in many places it was observed eroded as it were with small circular pits, sometimes shallow, and sometimes penetrating through the whole thickness of the false membrane. Occasionally these penetrating pits communicated with each other by sinuses or by a more extensive separation of the false membrane from the subjacent pleura; but at other times the ulceration penetrated through the pleura itself. When this happened on the costal pleura, it sometimes gave rise to caries of the adjacent ribs, and to external tumours, of which examples occurred in some of the preceding cases; but when it took place on the pulmonary pleura, a communication was at last formed with a bronchial tube, through which, according to the position of the body, part of the fluid escaped, or air entered.

After this membrane is formed and organized there is no hope of the lung again rising so as to fill the cavity, and the cure can only take place by the falling in of the side. How

far this can be assisted by operation is doubtful; but we have seen one case terminate in recovery, and another was materially relieved by diminishing the distension. At any rate, we have seen that the operation is attended with no inconvenience, and may at all times be safely tried even as a palliative means. The earlier the operation is performed the greater undoubtedly is the chance of success; and I regret that it was not had recourse to in the case of Hood, in whom the disease was of short standing, and the membrane was less dense than in other instances. But after the disease was fully recognized in this patient there was little hope of any advantage from treatment.

We have seen how often these diseases have existed for some time, even a very long time, without being detected, although when once suspected their diagnosis was always easy and certain. When indeed they exist on the left side, the change of the place where the heart is felt to pulsate is quite pathognomonic, and certain unusual feelings, which the patient often calls palpitation, indicate its commencement before it can be perceived by the medical attendant. When it occurs on the right side, the first symptoms are a sense of fulness, various affections of the stomach, and pains often referred to the hypochondriac or abdominal regions, so that the liver is not unfrequently considered to be the seat of the disease. When the effusion of fluid or the distension by air is very great, the racking pains are sometimes exquisite, and the side is enlarged by the displacement of the liver. In all cases of this kind the omission of examination by percussion and auscultation is a culpable neglect, as they never fail to ascertain the nature of the disease, and prevent the employment of unnecessary and hurtful remedies.

The mode of performing the mechanical actions of respiration is greatly altered in these diseases. It is obvious, that as soon as either of them is advanced so far as to render the diaphragm concave towards the thorax, and convex to the abdomen, the two halves of this muscle can no longer act similarly, and that the one-half becomes an antagonist to the other.

If the muscular fibres of the inverted half are capable of acting, their contraction will diminish the length of the thorax, and their relaxation increase it, so that were both sides to act simultaneously, the one side would be inspiring and the other expiring at the same moment. But it is more probable that in this morbid state they act alternately, (for muscles readily assume these variations in their catenated actions,) and that the one-half is contracting while the other is relaxing, and *vice versa*, so that inspiration and expiration take place in both sides of the chest at the same time. It is, however, still more probable that

when the disease is far advanced, the inverted side of the diaphragm does not act at all, but remains passive.

When the action of the diaphragm becomes disturbed, even the sound side of that muscle is less employed; and inspiration is chiefly performed by the cervical muscles of respiration, which elevate the whole chest. This was strikingly observable in Mr N. but it may be seen in every patient affected with empyema, especially as they generally respire only with one side. Indeed, it furnishes one of the most decisive diagnostic signs for those who are not habituated to the employment of percussion or auscultation. If the patient suspected to be thus affected be made to sit evenly upon a stool without a back, the increased elevation of the shoulder of the sound side contrasted with the absolute rest of the other leaves no doubt as to the existence of the disease.

ART. VIII.—*An Essay on the Medical Profession, showing its natural unity, and suggesting such arrangements as would render its condition conformable to just principles of Political Science, and conducive to the interests both of the Profession and the Public; being supplementary to two Essays on the same subject formerly published in the Edinburgh Medical and Surgical Journal* By EDWARD BARLOW, M. D. Bath.

To the Editors of the Medical and Surgical Journal.

THE attention of medical men appears at length aroused to the manifold evils to which defective organization of the profession has given rise, and the several departments seem laudably bent on endeavours to correct the errors and abuses from which they respectively suffer. At such a moment, while each department is engaged in supporting its own cause, it cannot be unseasonable to extend the inquiry into the claims which the profession has to a general revision of its system, with reference to the collective interests, and to inculcate once more the principles which should guide the legislature in improving its condition.

Expositions of this nature and extent are the more necessary, there being no superintending association fitted for taking those comprehensive views which the occasion requires; for while each department is represented by corporate bodies, to whom the care of its interests is confided, none exists for regulating the collective profession, adjusting the conflicting interests of its several branches, and adapting the energies of the whole

for the public good. The departments, in asserting their respective rights and interests, are necessarily limited by partial views. They conceive, with sufficient truth and accuracy, perhaps, what would be required to perfect each branch as an isolated establishment, but they fail to recognize the natural connexion which unites the whole, and are thus, even in their collective labours, incompetent to display the comprehensive views, or establish the just conclusions by which alone the legislature can be safely guided.

The object of the present tract is to support that cause which none of the departments can be expected to espouse, and again to direct attention to the natural unity of the profession, the intimate connexion which subsists among its branches, and the impracticability of adjusting any one successfully or equitably, without rendering its improvement compatible with the well-being of the whole, and with the public welfare.

These are the views which the statesman should contemplate. Partial regulations flowing from no fixed principle, and having no reference to that comprehensive policy which can alone guide the legislature aright in the discharge of its arduous and responsible duties, can lead only to injurious collision among those who ought to act in concert, and must ever fail to realize the purposes of public utility, for which all legislation is intended.

The subject is complicated, and presents difficulties which may account for the discordant opinions entertained respecting it. It admits, however, of being elucidated, so as not only to display the natural condition which the profession would have assumed, if not shackled at every step by forced and unwise restrictions, but also to determine the government which sound policy may now establish, so as to restore the natural unity, without violence to any of the departments into which it has, by the impolicy of centuries, been divided. Both inquiries are indispensable, the former as deducing the abstract truths to which legislation should incline, the latter as showing the modifications which existing circumstances demand. Without the former, attempts at improvement would have no sure guidance, while the latter is no less necessary for restraining the excursive flights to which speculation is prone, and for insuring that adaptation to what time has established, which in political reformations can never be safely disregarded. Were the wisdom of such policy clearly understood, civil government would redound more to the welfare of the subject, and the credit of the rulers, while contending factions would blend into one co-operating body of consistent and constitutional patriots. By resisting improvement, and clinging with superstitious reverence to the

errors of past ages, one party enforce an accumulation of evil, productive of much misery, and which hazards the very revolution, the apprehension of which deters them from change; while the other, by adhering too closely to speculative right, and not making due allowance for habits and tendencies which long practice has engendered, forego that sober judgment which would beget confidence in their wisdom, and swell the ranks of their opponents by numbers friendly to their cause, but who shrink from the rashness which their leaders, in the ardour of contention, too often betray. The distinction between the whigs and tories of the modern times is slight, and becoming every day fainter. No tory now contends for the divine right of kings, nor can a whig be found to question the just prerogative of the crown. The more moderate of both sides indeed approach so nearly, that the difference between them is more in degree than in kind, the one being more impressed with the advantages of progressive amelioration, the other more apprehensive of innovation, because it distrusts the power of limiting its extent. It is a feature of the times which every patriotic mind must hail with delight, that these long opposed bodies approximate so as to encourage a reasonable hope that the barbarous denominations by which they have been so long distinguished will in no long time cease to exist. These remarks are introduced with reluctance, for I am aware that many will deem them inapposite,—yet their pertinency, I trust, will become manifest as I proceed,—for as any adequate or effectual reformation of the profession (and some it greatly needs) must assail the errors and abuses of chartered bodies, before whom antiquity holds her venerable shield, it is to be apprehended that evils, however glaring, may be upheld from mere timidity at interfering with what has long subsisted, and the best interests of the present generation be sacrificed to an excessive respect for what our well-intentioned, but less enlightened ancestors, had centuries ago decreed. Were knowledge and civilization stationary, these antiquated provisions might continue to serve the purposes for which they were designed; but being progressive, and having advanced within the last half century with a rapidity unexampled, it is surely no political heresy to doubt the fitness of enactments framed in the fifteenth century for regulating the affairs of the eighteenth.

Having on former occasions amply discussed the general state of the profession, as well as the particular condition of that department which constitutes the province of the physician, it remains only to show how the organization now required may be reconciled with the principles illustrated, yet so accommodated to the present state both of the profession and of society as to in-

volve no infringement of either individual or corporate rights, from which the most timid need recoil.

In an essay published in the fourteenth volume of the Edinburgh Medical and Surgical Journal, entitled "An Attempt to Develop the Fundamental Principles which should guide the Legislature in Regulating the Profession of Physic," I have traced the history of the profession from a remote period, and deduced from well-established facts the tendencies to which it naturally inclines. I have shown that the public bias has always been in favour of the practitioner who combined in his own person the greatest number of the several departments; and that this collective exercise of professional functions has always prevailed, though opposed on every hand by chartered and legislative restrictions. The general practitioner has in fact been created by the public, and forced into an acknowledged existence in despite of every obstacle; for while surgeons invariably embraced medical practice, and apothecaries practised widely in all branches, the union of offices was for a long period not only unacknowledged, but even disavowed by those who profited by the combination. Under the irresistible impulse of public determination this reserve insensibly gave way, and at length, men of high reputation and acquirements did not disdain to avow themselves general practitioners. Still was this most important office rather suffered than sanctioned, while the public influence in establishing it never ceased to operate. Surgeons, in order to qualify themselves for discharging its duties, combined pharmacy with their other pursuits, unrestricted by their college, although the combination has never been formally recognized by this body, nor has any inquiry into pharmaceutical competency formed a part of the college examination. Apothecaries too supplied to themselves the deficiencies of their early education, superadding some knowledge of physic and surgery to pharmaceutical attainments, thus elevating themselves to the rank of general practitioners, of which class they constituted a large, respectable, and increasing portion.

It is curious that though the office of general practitioner has always prevailed, so successful were established institutions and prescriptive usages in repressing all acknowledgment of its existence, that the denomination now so generally received is but of recent date. So imperceptibly has it crept into use that it would be no easy matter to fix the period of its recognized adoption. At first surgeons extended their practice to physic, and apothecaries widened their sphere of utility covertly and as it were by stealth, hardly acknowledging what in those days would have been deemed an unauthorized trespass,—disavowing the combination if charged with the imputed irregularity,

and content to profit by an exercise of talent so founded on reason and public utility, without claiming the credit which their subserviency to public exigencies undoubtedly merited. Emboldened by success, and confident of public support, this useful body now glory in an appellation of which they have no cause to be ashamed.

It appears that the body of general practitioners was derived from different sources, consisting of surgeons who combined pharmacy with their other pursuits, and of apothecaries qualified in surgery and physic. Had the surgeons profited by the truths here developed, and foregoing narrow prejudices, exercised an enlightened foresight, they had it in their power to occupy this field to the effectual exclusion of their rivals. By judicious exercise of the authority derived from their charter, by a candid acknowledgment of the necessity for general practitioners, with a determination to qualify their own licentiates for the important trust, they, as possessing advantages with which the apothecaries could not compete, might have engrossed the supply with little resistance. In other words, if pharmacy had been included in the discipline of the college of surgeons, and just encouragement given to its exercise, young men preparing for practice would have given exclusive preference to this mode of admission, and the apothecaries would have insensibly declined. Shallow policy, however, with some portion of vain pride, led this corporation to pursue a different course. They did not indeed actually forbid the practice of pharmacy to their licentiates, for this would not have been endured, but they gave no countenance to the conjunction. Fearful of recognizing what they weakly conceived a degradation, they abstained in their examinations and introductory courses of study from all allusion to practical pharmacy, though well aware that it was an essential requisite to by far the greater portion of those who underwent their ordeal.

They went further, and affixed a direct stigma on the combination of pharmacy by excluding from the offices of the college all who practised it ; an ordinance which appears no less illegal than impolitic, yet which is in force to the present day. The advantage in their actual possession being thus disdained by the surgeons, the apothecaries failed not to profit by the error, taking undisputed possession of the abandoned ground, and at length entrenching themselves within legal powers more complete and efficient than are possessed by any other medical corporation. I need hardly refer to the apothecaries' act passed in 1815, by which this body has acquired exclusive control over pharmacy, and is empowered to grant qualifications both in me-

dical and surgical practice to an extent surpassing any authority which the College of surgeons is entitled to exercise.

The course of events pourtrayed in the preceding pages, and in my former essays, merits deep consideration, as including the essential principles on which the whole profession should be organized, and without conformity to which no arrangement can be permanent or efficient. It has been shown that the first disunion of physic and surgery sprung from causes unconnected with public utility or professional expediency, being solely influenced by ecclesiastical restrictions which forbid the monks from practising many branches both of physic and surgery. When prohibited by a papal bull from leaving their cloisters for the purpose of attending the sick, these reverend physicians prescribed at home, holding intercourse with those who could not attend them, through the medium of their servants, to whom, being also their barbers, and thus possessed of some manual dexterity, the superintendence of surgical ailments was entrusted. Hence arose the class of practitioners, afterwards sufficiently celebrated throughout Europe, the Barber-Surgeons.

The progress of surgery from this period has been already traced in a former essay, and need not be recapitulated here. As might be anticipated from the importance of the functions discharged, and the indispensable necessity the public were under of resorting to surgeons for professional aid, this body gradually advanced in respectability and public favour, until the French court rescued them from their degraded state, established a college for their use, and gave them a separate and independent constitution. Later advances are familiarly known, and need not be particularized.

While surgery nobly sustained its elevated position by signal improvements in its immediate branch of science, and though surgeons were ever widely engaged in medical practice, a re-union with physic, though called for by every dictate of experience and common sense, has yet never been attempted. For this many causes may be assigned. The profession had insensibly settled into subdivisions, which, sanctioned by time, and interwoven with public habits and prejudices, acquired a prescriptive authority, which it would have been deemed almost sacrilegious to dispute. It was supported, therefore, in its forced and unnatural constitution by all who hold mere antiquity in reverence; and they who are thus influenced are ever a numerous body. More inquiring minds, deluded by the advantages resulting from the division of labour in the mechanical arts, were misled into a persuasion that the distinctions in physic must have sprung from some similar influence, and gratuitously admitted what they were at no pains to investigate. Had they

traced these distinctions both in their causes and their effects, they would soon have discovered that neither evinced the slightest connexion with the principle which had dazzled their imaginations. In no inherent necessity for subdivision did these distinctions arise; no public or professional good has been attained by their maintenance; on the contrary, it can be clearly shown that their existence has been prejudicial both to the public welfare and the progress of science, and that it has been solely owing to the public will, exercised in defiance of corporate rights and legislative prohibitions, that injury far more signal has not resulted. I may be thought to urge this point too forcibly, but my best judgment impels me to press it; for unless the principle here maintained of the natural unity of the profession be clearly understood and unequivocally admitted, it will be impossible to advance one step in those ulterior considerations which the present inquiry calls for. Indeed, the first axiom laid down for all legislators attempting to regulate the profession should be, that it is one and indivisible. Lest this declaration should appear too revolutionary, and beget a cautious distrust of what is to follow, I may here in self defence, and to conciliate my more timid readers, assure them that I meditate no revolutionary changes; and I trust I shall speedily show that the provisions which I contemplate involve no extinction of existing institutions, but that, in the modifications which I suggest, I recognize every existing right, and pay due deference even to prejudices, which, though nurtured in ignorance, have acquired a sanction from the length of time during which they have endured.

Having on former occasions confirmed this alleged unity by showing the actual identity of qualification required from every branch for a conscientious discharge of their respective duties,—having proved that a physician, ignorant of surgery, and a surgeon who is not well versed in medical science, are equally dangerous practitioners,—I may safely assume the collective evidences on this point as complete, and proceed to the more immediate object of this essay, namely, the provisions now required from the legislature for giving to the whole profession that organization, which, corresponding with its natural tendencies, and conducing both to the public welfare and extension of science, would give it the highest attainable perfection as a branch of civil polity.

The natural unity, then, being thus assumed, it may, by impatient inquirers, be asked, would I give to the profession now, that form which abstract reason would indicate? To this I unhesitatingly answer, No; for I know of no instance in which abstract principles alone can be considered as safe guides in reforming

political errors or abuses. There is a renitency in all social institutions which unfits them for great or sudden changes, and which it is eminently unwise in reformists to overlook or disregard. With error, however obvious, or from whatever source it spring, there always becomes intermixed some portion of redeeming good. The body politic, as well as the natural body, possesses within itself conservative properties, which accommodate it to every circumstance and situation, and by some counteracting influence weakened or render harmless what it cannot correct or remove. Thus we continually see speculative truth apparently falsified by experience, and establishments which could be theoretically proved unsound, evincing practically all the requisites of health and vigour. If any such evidences of practical benefit could be adduced in favour of the present state of the medical profession, I would readily wave all theoretical objections and acknowledge that the labour of more than twenty years, during which this subject has engaged my thoughts, has been misapplied. If I do not greatly err, however, I have in my former tracts abundantly proved, not only that the present organization of the profession is both defective and erroneous in principle, but that, in its practical results, it is a fruitful source of mischievous evils.

I come at length to consider the present state of the profession, and the modifications, which, according to the foregoing views, it essentially requires.

The course of events, and the influence of political ordinances, have caused the profession to consist of physicians, surgeons, and apothecaries. To these we may now add druggists, who are but the apothecaries of days gone by. It has been shown that from the surgeons and apothecaries is derived that body the most extensively employed of any, the general practitioners. The great end of both the surgeons' and apothecaries' corporate institutions being now to supply general practitioners, it seems vain to make a distinction between them, for no essential difference exists. All surgeons may, if they please, be general practitioners, and all are so, even to the highest, so far as regards the combination of medical practice. If some refrain from pharmacy it is from motives of a purely personal nature, in which the public have no concern, and which are not of sufficient moment to justify a distinction in all other respects vain and groundless. It will simplify our subject, therefore, to consider practitioners in physic as consisting of two classes only, physicians and general practitioners. It may here be remarked, that, consistently with the axiom laid down, there should be one class only; but such a conclusion would involve the error formerly deprecated,

for the ordinances of centuries, with the effects on society resulting from them, are not thus to be set aside. The several classes specified have long existed, and the public has upheld them. To the public voice, especially in a matter so directly connected with their personal interests, every deference is due, and on this ground alone I could be content to rest the claims of the physicians for continuance as a separate branch, and for full support from the legislature in those rights to which they are entitled. I mean not, however, to trust their cause to this single issue, nor do I shrink from defending, on the best principles of reason, that support which the public has given them. The principle of unity so much insisted on undoubtedly requires that the physician should possess surgical knowledge as well as medical; but he is under no necessity to practise both. In this respect he must be influenced solely by the demand for his labour. If he prefer medical practice alone, and the public are pleased to give him adequate employment, I see no disadvantage in the more limited range to which he confines himself; and to the public necessity as evinced by the demand may this matter be safely trusted. So long as the public, by employing physicians, while free to give its preference elsewhere, evinces a want of their services, should these services be supplied. When they cease to be needed, the effects will soon appear in the gradual secedence of physicians and the substitution of more favoured practitioners. In maintaining competition the physicians should have all just privileges, but no exclusive rights; and the public, when free to select the practitioner who best serves their purpose, will be little likely to form an erroneous judgment. The final decision at least must rest with them, and it will be exercised in defiance of all legal prohibition or constraint. The will of the public, though slow, is yet sure to counteract ordinances adverse to its well-being; by this gradual and imperceptible progress it is, that time, the great innovator, effects so many revolutions, and one marked advantage of changes thus induced, over forced or arbitrary reformations, is that the frame of civil society is not rudely disturbed, nor any individual injustice inflicted.

I have said that the public have upheld the physicians. I should have added that they have done so under circumstances which, but for this support, must have sunk them into annihilation. And here I beg leave to direct attention to another tract of mine, also published in the *Edinburgh Medical and Surgical Journal*, Vol. xvi. p. 479, entitled "An Exposition of the present state of the Profession of Physic in England, and of the Laws enacted for its Government." From this it will be seen what the physicians of England have had to contend

with; and when the extent and continuance of oppression to which they have been subjected are considered, it must be allowed that they have borne their injuries meekly. So meekly, indeed, that even the right to complain seems from disuse to have become questionable,—and if they come forward to expose the oppression under which they have long and injuriously suffered, or to seek the restitution of rights from which they have been for centuries debarred, they are met by some flippant opponent among the general practitioners with imputations of arrogance and selfishness, and their efforts of pure self-protection are misconstrued into unwarrantable encroachment on the assumed rights of this rival body! Such petty hostility it may be unworthy to notice,—and I am far from imputing such disposition generally to the respectable class to whom I refer. All, however, who are familiar with the floating literature of the times must have noticed the petty warfare which has long been carried on against the physicians by men who, proud of their exaltation, and boasting of their superiority until they actually believe in its reality, affect to look down on the physicians as having no just pretension to public favour or support. All this may be, and I dare say is very harmless, and the physicians in treating it with dignified silence have perhaps acted wisely. Still in this reading age, when the periodic press exerts such extensive influence, it may not be safe to condemn even such weak assailants, or wholly to refrain from those vindications which even groundless aspersions, when boldly uttered, may demand, but which conscious integrity too often neglects. The disposition to decry the physicians, though of late years more boldly displayed, is yet not of recent date. Garth had occasion to notice it in his day, as appears from the following stanza:—

“ Thus modern 'pothecaries taught the art,
From doctors' bills to play the doctors part,
Bold in the practice of mistaken rules,
Prescribe, apply, and call their masters fools.”

The general practitioner of the present day is, however, a very different being from the apothecary of Garth, and may safely trust to his sure hold on public confidence without any attempt to promote his own elevation by the physicians' abasement. In him such jealousy is every way unworthy; and I have made the foregoing remarks not with any feeling of resentment against a body whose cause I have myself pleaded more forcibly than has been done by any member of their own class, and who possess my respect, but in the hope that this appeal to their better feelings may have the effect of silencing a strain of calumny and invective which, if pursued, can only reflect discredit on those who employ it. I gladly return from this digression to the more immediate object of inquiry.

The scrutiny into the precise nature of the respective classes was necessary, in order to remove an error, which, if suffered to continue, must obscure all reasoning and vitiate the conclusions deducible. It is seen that no inherent necessity exists for any distinction, but that, for all that pure reason can advance to the contrary, the functions of the profession might all be administered by the same persons, and a single political institution embrace the whole. Physicians might practise surgery, and would in many instances do so in self protection but for the prejudices so long maintained against this combination,—the jealousy of rivals who, though they trespass without compunction on the physicians' department, are acutely sensitive to any supposed encroachment on their own,—and the vain pride of their own body, engendered in ignorance and misconception of what real respectability requires, and confirmed by the denunciations against all contact with surgery or pharmacy issued by their imperious and aristocratic rulers. No person practising pharmacy will be received into even the degraded class of licentiates of the London college; and if a surgeon seek admission, even though well-grounded in medical science, and possessed of a university degree, he must abjure his early profession, and actually have his name expunged from the surgical college to which he belongs, ere the college of physicians will condescend to enrol him, not as a member of their body, but as an humble appendage, to whom the practice of physic is, at least in part, graciously permitted. That surgeons practise physic extensively,—that it forms by far the greater portion of the general practitioner's occupation—and that were surgery alone depended on, it could not give common subsistence to the hundredth part of those who profess surgery, are facts too well known, and too generally admitted to need further argument. Where then does the distinction lie, and what reason can be assigned for opposing legal or corporate restrictions to any combination of offices, to which the good of the public, and the convenience of practitioners may incline?

The only reason for the continuance of any distinction is founded on the state of the profession which is found to prevail,—on the extent to which the respective branches actually exist,—and in the public feeling on this point, which, however engendered, has now been confirmed by the habits of ages, and ought not to be rudely disturbed. These considerations are of sufficient weight to demand attention, and are such as the enlightened statesman will not overlook. It is the essence of sound legislation to abide by guidance of principles, yet so to modify their application, that no existing right shall be violated, or any established usage unnecessarily outraged. Of all policy the most hazardous by far is that too ardent pursuit of speculative perfection,

which, regardless of the obstacles that ages have accumulated, and of the evils inflicted by their incautious removal, hastens with rash impetuosity to seize the object of too vivid conception. The error requires to be the more carefully guarded against, because among visionaries of this class some of the best and purest minds are to be found, who, dwelling with delight on the excellence which a lively imagination portrays in glowing colours, closing their eyes to the imperfection inseparable from all mortal affairs, and impelled by feelings which they are unused to distrust, either overlook the impediments in their way, or, with pliant integrity, deem the sacrifices involved, and the injustice inflicted, as amply redeemed by the signal good which they aim at realizing.

In the present instance it fortunately happens that unerring principles may be strictly applied, and yet no violence be committed against any interests important either to the profession or the public, or at all worthy of being preserved. The principle of unity may be maintained to the utmost extent necessary, by applying it to the primary qualification of practitioners, physicians being required to ground themselves in surgery, and surgeons to possess an adequate knowledge of every branch. With this simple and obvious provision, the propriety of which cannot be disputed, the distinction of practitioners is harmless, and may with perfect safety be left undisturbed, provided that each class be left free to compete with the other, and that it be left to the public, who alone are competent judges, to decide what preference may be due to either. It is the business of the legislature to provide that all who profess the practice of physic shall be duly qualified; and this object insured, the profession and its departments may be safely left to find their natural level. No legislature, however enlightened, can have a right to say to the public you shall employ physicians instead of surgeons, or the contrary. The establishment of a natural distinction, if such existed, or the restoration of a natural unity in the practice of the art, is a question not now to be discussed, for time and the course of events have long since decided it. The only rational object now is, so to administer what is actually established, that it shall not have to encounter the counteracting influence of principles founded in nature, deducible by sound induction, ever operating and uncontrollable. Principle demands that the natural unity of the profession should not be violated by authoritative constraint or restriction,—hence the primary qualification of all practitioners should be substantially identical. Regard for existing order requires, that whatever is established should be disturbed as little as is compatible with the public welfare.

Let us now proceed to inquire how far the profession, as at present arranged, admits of being organized so as to keep both objects steadily in view. In discussing the subject it will simplify inquiry to regard the profession as consisting of two classes only, namely, physicians and general practitioners, waving at the outset all separate consideration of surgeons and apothecaries. It has, I trust, been clearly shown, that between these latter no essential difference exists, both discharging the same functions, and the corporate superintendence of both having for its main object the supply of general practitioners. As thus identical, I can use no better term to designate them in the capacity common to both than that which I employ, and which, though as yet sanctioned by common parlance only, is sufficiently expressive and recognized to justify its adoption. Of each class it will be necessary to scrutinise both its course of education, with the qualifications necessary for fulfilling its functions, and the protection which, in the exercise of them, it ought to receive from the state.

It will assist this inquiry, if, previous to the special examination, a more intimate view be taken of the department occupied by the physicians. Their continuance as a class of medical practitioners is vindicated, on the broad ground of their being actually, though not exclusively, in possession of this department, and on the unequivocal demand of the public for their services, as evinced by a support which has so long upheld them, under circumstances of oppression, neglect, and formidable competition, which must otherwise have annihilated them. By what inherent qualities they have been able to maintain this struggle, and preserve the confidence which the public have reposed in them, it must be interesting to investigate as a matter of mere curiosity. It is of the first importance to do so when the means of sustaining these qualities by suitable education, and the hazard of bartering them for some speculative refinements, are subjects under consideration. That some such qualities exist might be inferred from the success of the physicians having been independent of all legal support or exclusive privilege; for though within their department monopoly has been exercised, it has been for the benefit of a few, and in opposition to the great body of physicians, while none has been attempted against their aspiring rivals of the lower departments. No inference, therefore, can be more conclusive than that the public favour enjoyed by the physicians under actual excommunication by the chartered company, originally incorporated for their protection and support, and the opposition of a body of active and intelligent rivals, must have resulted from some

inherent qualities by which the public have been benefited, and which the public interests require to be preserved. It has been an advantage, that, owing to the rank in life, both social and professional, which physicians have enjoyed, classes of the community have been induced to enter the profession who would have recoiled from any less dignified association. Mental cultivation, liberal sentiments, and polished manners, have therefore distinguished this class to an extent that would have been unattainable, if the individuals composing it had been of humbler origin. A good has hence resulted which is not to be despised, in the facility with which the profession has accommodated itself to the gradation of civil society. The intercourses, both social and intellectual, that have been thus maintained, have conferred dignity on the medical character, and shed a lustre on the profession which has radiated to its humblest ranks; and they who now assail the physician and seek to supplant him are little aware how much they would themselves lose by their ambition being gratified; for with all due respect for the usefulness of the general practitioners, (and that I do not undervalue them my preceding essays abundantly prove,) I trust I may stand acquitted of arrogance or presumption, if I allege that in mental cultivation and liberal science they, as a class, are inferior to the physicians. That there are highly cultivated general practitioners, as well as ignorant physicians, I am well aware, but the admission goes no way towards falsifying the general assertion. The late Dr Parr was no contemptible judge of mental attainments, and he has left on record a testimony which amply supports the pretensions here affirmed. I quote from a note in Dr Buchan's *Bionomia*, not having by me Dr Percival's *Medical Ethics*, from which the passages are originally taken. They were contained in a letter addressed by Dr Parr to Dr Percival.

“ I have long been in the habit of reading on medical subjects, and the great advantage I have derived from this circumstance is, that I have found opportunities for conversation and friendship with a class of men, whom, after a long and attentive survey of literary characters, I hold to be the most enlightened professional persons in the whole circle of human arts and sciences.”

How far this eulogy would have been merited if the profession had consisted solely of general practitioners may well be doubted. But it may be remarked, that it is applied to the medical character generally, and without direct reference to physicians. Dr Parr has again expressed himself with less ambiguity, and I again quote his words. “ Of the three liberal professions, I must confess that in erudition, in science, in habits of deep and comprehensive thinking, the pre-eminence in some

degree must be assigned to the physicians." That such attainments must conduce to the advancement of medical science will hardly be questioned, while the liberal feeling inseparable from the philosophic spirit, so unequivocally attested in the foregoing declaration, must exert an influence on the profession of the best and purest kind, elevating its character, and preserving it from degenerating into a sordid trade.

Of such qualities the public have sufficient acumen to form some estimate, however incompetent they may be to appreciate fully merits so imperfectly cognizable by ordinary apprehension; and to the conviction of superiority thus impressed must be ascribed the steady support which physicians have so long received. I trust I have stated enough to justify the continuance of this body as a class of the profession.

Assuming, then, that the physicians are still worthy of holding the place so long assigned to them, I proceed to consider the qualifications which they ought to possess as candidates for practice, and the legal protection to which they are entitled. A similar consideration of the class of general practitioners will then close the present essay. In what is to follow I may be brief, for so amply have both topics been discussed in the tracts formerly published, and to which, as contained in your own Journal, your readers must have ready access, that it will suffice on the present occasion to refer to what has been already detailed respecting them, so as to limit the following remarks to that application of principles in a just organization of the profession, for which the present essay is chiefly designed.

In the qualification of physicians the only innovation I have to propose is, that a competent knowledge of surgery should form an integral part. The necessity for them possessing such knowledge is so obvious that I need not here dwell on it. My object in requiring it is not to enable them to practise surgery, but to render their competency in their own department more complete. That physicians will practise surgery, and thus identify themselves with the general practitioners, is not to be apprehended. Their claim to public patronage rests on far different qualities, as has been already explained; and, impressed as they must be with this conviction, they are little likely to abandon their own proper station, or divest themselves of their distinctive character, by engaging in surgical practice. With the proviso of this addition to their attested knowledge, I would conceive it hazardous in the extreme to tamper with these modes of education so long and so successfully pursued, and which time, experience, and talent have matured, by aiming at a speculative perfection plausibly urged, but wanting the best test of political excellence, that of its advantages having

been practically demonstrated. I can conceive nothing more pernicious in political regeneration than to form a *beau idéal*, and proceed to realize it at all hazards, regardless of the wreck of established order, and of the inevitably attendant mischief which the accomplishment of such visionary schemes must ever occasion.

Our physicians have heretofore been derived from various sources, with very unequal qualifications. It would undoubtedly be desirable so to assimilate the courses of education pursued in our medical seminaries, as to remove all disparity, but such as natural inequality of talent must ever impart. In establishing a national qualification, the model should be taken, not from the mere creations of fancy, but from what experience has actually proved to be efficient, supplying whatever may be rationally devised to render it more perfect, yet avoiding all revolutionary changes. And here we have happily both beacons to guide our course, and rocks to shun. We have schools of physic in various stages of advancement. Some have been long stationary, neglecting to keep pace with science and social improvement, by supplying the deficiencies thence resulting. The neglect into which they have fallen as medical seminaries affords ample proof of their consequent incompetency. Others profiting by the advancement of knowledge, and conforming to the wants of society, have so framed their courses of instruction as to comprise the essential objects for which medical education is designed, and to them the tide of public favour has irresistibly flowed, although the preference involved a sacrifice of privileges, which a less perfect course of education would have conferred. The English universities, with those of St Andrews and Aberdeen, are instances of the former,—those of Edinburgh and Glasgow of the latter. The rejected universities required high classical attainments from candidates for medical honours, passing lightly over those acquirements which should form the basis of a medical degree. Edinburgh and Glasgow, with more good sense, gave medical science and literature a preponderance, yet without neglecting that liberal cultivation in general literature which should grace the professional character.

The extent to which general literature and science should be enforced in a well-regulated system of medical qualification is a point on which no two minds perhaps will precisely agree. It is clear, I think, that they should not be insisted on to the necessary neglect of medical cultivation, for time and human capacity have their limits. No one mind can possess universal knowledge; and if both time and faculties be engrossed by general knowledge, professional attainments must be incomplete. There is a hazard, too, of making the standard

of the general knowledge too high, lest the necessary supply of practitioners be thereby limited, and the public wants be inadequately provided for. On every account great caution should be exercised in deviating from what long experience has established, and no change should be hazarded on merely speculative grounds. To justify any, full proof should be given not only of the literary and scientific attainments of physicians being deficient, but of actual injury being sustained in consequence by the public. If no such deficiency can be shown, no such injury demonstrated, then may the established course of education be deemed as perfect as the nature of human institutions will permit, and interference in this respect be dispensed with. Proof of liberal education should certainly be required at some period of the physician's under-graduate course, but simple examination would afford this; the possession of adequate knowledge thus attested should suffice, and it would be eminently impolitic to require that it should be obtained in any prescribed way. To demand that a literary degree in arts should precede the course of medical instruction, or be essential to a medical degree, would be a sure means of impairing the medical competency of the individual, instead of rendering his qualification more complete. It should not be forgotten that physic, as well as a science, is a practical art.—that the necessary familiarity with the phenomena of health and disease, (the foundation of all practical knowledge,) can only be acquired by diligent application in early life, and that the previous attainment of a literary degree would absorb both time and attention, which a course of medical education cannot spare. To enter late on the study of any profession is a disadvantage which no ordinary diligence or acumen can afterwards redeem; and it is well known that those physicians who have superadded a medical degree to a literary one have not been always the best practitioners.

Of all modes of insuring literary qualification, that of insisting on a literary degree would perhaps be the most objectionable, as giving a monopoly to those who were within reach of this requisite, and excluding all who, from locality or other causes, could not conveniently attain it. When the limited capacity of our universities, too, with the utter impracticability of all who are destined for the profession of physic procuring admission is considered, to require this test of literary attainments must appear both absurd and unjust. The effect would clearly be to confine the supply of medical graduates to particular districts, while it may be doubted that a single advantage would result from so violent a change. In the present day candidates for this department are little likely to enter on their career with any great defects in literary preparation. Im-

provement in general education, too, is rapidly progressive, and this alone furnishes a strong security against ignorance, while a farther and more direct one exists in the motives which impel candidates to enter this department. It has been shown to derive its distinctive character as a separate class chiefly from superior cultivation in literature and science, and thence to acquire that hold of public favour which has so long upheld it. Is this indispensable advantage likely to be disregarded by those who give this department a deliberate preference, and who, if they neglect it, must signally fail? If to attain practice with the least literary cultivation be the object, will not the department of general practice be preferred, as leading to much more sure and speedy success, and may not the embryo physician, therefore, be safely trusted to the powerful influence of self-interest, for not neglecting that on which all his hopes of successful competition must depend? Every day the general practitioners are becoming more enlightened and better cultivated. Physicians can only hope to compete with them by keeping ahead in mental cultivation. To neglect this would be a *felo de se*, which they are not likely to commit, and therefore the necessity thence arising appears to me a far better security against ignorance than the forced acquirement of any literary honours, however distinguished.

What are the literary and scientific attainments indispensable for graduates in physic should be determined by the medical seminaries that have best succeeded, and in laying down a qualification to be adopted by all medical schools the universities granting medical degrees might be suffered to decide. My own views lead me to see much greater hazard in enforcing literary qualification, adjusted by a scale of speculative excellence, than in trusting much to the intelligence and self-interest of the parties concerned. I have pursued this inquiry the more freely, because certain proceedings connected with the late Royal Commission deputed to investigate the state of the Scottish universities beget an apprehension that the report of that body will be favourable to such extensive and literary cultivation in medical education as would assimilate the best and most approved schools of physic to those antiquated systems, the insufficiency of which has led to the utter desertion of the universities that have so pertinaciously maintained them,—a solecism in politics which it would be difficult to defend.

Having disposed of the qualification of physicians so far as my present purpose requires, I come next to consider the legal protection to which they are entitled in the exercise of their functions. And here, again, I must refer to my former essay, in which the history and present state of this department, the

laws by which it is governed, and the mode in which these laws have been administered, have been clearly and fully discussed. It must excite surprise that this truly important branch of civil polity has, so far as regards the physician's department, undergone no revision since the sixteenth century, a period when medical science was in its infancy, and any thing bordering on a regular profession only creeping into existence. The inadequacy of laws framed under such circumstances, for regulating the profession at the present day, might be safely inferred, even if no proofs were adduced of their utter incompetency, or of the gross abuses that have been practised under them. In the tract to which I have just referred, however, evidences sufficient to convince the most sceptical, furnish proof, that, however deficient these antiquated statutes, the profession has suffered not so much from their imperfections, as from the total abandonment of their provisions by the London college, and the substitution of an arbitrary and illegal system, resting on no authority but that of by-laws enacted by themselves. Indeed, so widely has this corporate body departed from every discipline prescribed by its own charters, and even from the denomination given to it by these charters, and recognized by the statutes of Henry VIII. and of Mary, that its identity with the body to which chartered rights and legislative powers were granted comes at length to be formally disputed,—and it is questionable whether the College, as now constituted, could establish a right to sue in the courts of law, or exercise any other privilege whatever under the statutes mentioned. If the perfect nullity of the college should be thus proved, the legislature need have little respect for asserted rights, founded in usurpation, injustice, and selfishness, and which can be clearly shown to have inflicted signal injury both upon the profession and the public.

Freed from these trammels, the legislature may proceed, without hesitation, to remodel the superintendence for guarding the rights of physicians, and adopting their services for the public good, in which they have so simple a task to perform that they cannot err. Physicians being duly qualified by the universities, some corporate institution is necessary for embodying them so as to protect this department from unqualified intrusion; and, so far as professional interests or the welfare of the public are concerned, to this single function should corporate authority be restricted. To verify degrees, and enrol the possessors as members of the body corporate, entitled when thus admitted to a full participation of all privileges granted by the statute of incorporation, is the only control they should be suffered to exercise. Examination of candidates must ever be improperly placed when confided to such hands. For such

ordeal there is now no pretext. When a college was first formed to preside over the profession, no medical schools existed in Britain, and the examination by the college was then a necessary task. To continue it now that medical schools abound, and that the qualification of physicians is vouched for by universities having medical schools attached, is a solecism no less pernicious than absurd, the mischief of which is not redeemed by a single advantage. It must seem strange that the glaring errors and defects of legal provisions have not, in the course of three centuries, led this chartered body itself to call for some revision of the law. But the explanation of this apparent supineness lies on the surface. Possessed of power they had no wish to relinquish it. They could not but know that what they exercised was stretched greatly beyond the law,—and they prudently abstained from bringing in question an authority so tamely submitted to, by any attempt to modify or extend it. This is the secret of that quiescence which the London college has maintained. Extension of power they wished, and often meditated; but they have been deterred from the rash attempt solely by their inability to sustain the scrutiny to which an appeal to parliament for fresh powers must have subjected them. They preferred a close and self-constituted divan to a representative government, and clung to imperfection rather than risk the restitution of rights to those over whom they had so long domineered.

In giving my opinion freely and boldly of this college, I wish to be considered as casting no reflection on the individuals composing it—as individuals they have my respect, and I can even make great allowance for their corporate delinquencies. The early departures from strictly legal right were no doubt adopted from some presumed advantage distinct from self-interest, and the remote consequences of which were not foreseen. Even the extent of power conferred by the statute might have been misconceived, and violation of right practised without consciousness of the deviation. Abuses, when accumulated and long submitted to, acquired a prescriptive force which they who exercised them were little likely to distrust or weaken, and to which the successive members so tardily and scantily added to this body must almost of necessity succumb. The formation of the executive body was but ill calculated to insure an enlightened administration of its duties; for all authority was usurped by the fellows, and individuals were elected not from peculiar fitness, nor by the suffrages of their professional brethren, but for the mere accident of education. But were this even otherwise, it must be long ere a new fellow comes to comprehend the real nature or legal extent of the

powers which he is called on to exercise. Unsuspicious of abused authority he may never think of scrutinizing its foundations, and may even close his career in utter ignorance of the usurpation to which he had been so long a party. If diligent to investigate, and of integrity so pure as to recoil from the injustice practised under the alleged sanction of abused and violated laws, he meditate opposition, the hopelessness of contending alone against established power may well appal him; and whatever his wishes, he must be content to abide by what he can see no prospect of reforming. In time even the same individual may, without impeachment of integrity, alter his views, and distrust his more liberal sentiments. Many honest prejudices must be enlisted on the side of what he finds long sanctioned and established; the system appears to his views to work well; and finally, he finds his own interests served by its continuance, a consideration which, however unconsciously, weighs with most men. To reproach the fellows of this college, then, with not relinquishing, for the good of the profession, power handed down to them by their predecessors, and apparently unquestioned, is both unreasonable and unjust,—and I deprecate every attempt to charge on individuals a corruption, which may yet be brought home to the collective body. So far from implicating individual members in the blame deserved, I am disposed to accord high praise for liberal and disinterested views, when I look to the extent of those minorities by which measures of narrow policy have been so often opposed. It is creditable to the fellows of this college that there has always existed a large number to advocate liberal principles, and resist those procedures which narrowed the institution; and if abuses have been suffered, the fault is less chargeable on the college than on that supineness on the part of those aggrieved, which neglected to trace existing evils to their source, and to call on the proper authorities for their correction. Such call must eventually be made, and to it the legislature will I doubt not give respectful attention. In pursuing it I trust that no bitterness of feeling against individuals will be suffered to vitiate the motives from which reform should be sought, nor to mar that amelioration of the profession, which a temperate and respectful developement of the evils under which it suffers will be sure to obtain.

The extent of the united kingdom requires that a medical corporation of superintendence should exist in each capital, among which there should exist a perfect community of privilege, so that individual members, being all of equal qualification, should be free to practice in whatever district personal interest might lead them to select.

The department of the General Practitioner may be dismissed with very few observations. The two branches of surgeons and apothecaries being now perfectly identical in the functions discharged, no purpose whatever seems answered by their preserving a distinct existence, nor can any good reason be assigned why they should not be forthwith consolidated. If the surgeons murmur at the suggestion, they should recollect that the fault of a rival and now co-equal body being called into existence is their own. They had full possession of the department of general practice which through false pride they disdained to maintain. They had it in their power to supply general practitioners to any extent. By treating the combination of pharmacy with unmerited contempt they have forced the apothecaries into a rank and importance in the profession which they could never have otherwise attained. A want existed which the apothecaries have supplied, and for this they are entitled to the thanks of the public. Their statute of incorporation has given them an authority as general practitioners, which cannot now be superseded; and I can conceive no possible expedient for getting rid of the complexity thus introduced, but that of consolidating both the surgeons and apothecaries in one comprehensive institution.

Indeed, the business of re-union might be carried still farther,—for, as the corporations of physicians and of surgeons, or general practitioners, would have respectively but very simple duties to perform, it is not easy to understand why they might not be united in one medical institution for each department of the kingdom. The respective branches might attend separately to the special duties which each had to perform, while they might collectively discharge other contingent duties, by which the public interests would be materially served. Such an institution might be rendered of great value by supplying many wants for which the state should provide. It might endow professorships to insure the public against any deficiency of public or private teaching,—form a national library,—erect a national museum of anatomy, natural, morbid, and comparative, and of natural history generally,—publish a national pharmacopœia,—provide a popular pharmacopœia as a substitute for quackery,—and finally constitute a court of appeal to which all questions involving medical truths might be referred. A particular advantage would result from such union in the greater perfection of the national pharmacopœia. Although a separate body has long presided over pharmacy, yet our ancient laws give to the college of physicians a paramount control. On them devolves the office of revising and publishing the national pharmacopœia. This requires an intimate acquaintance with che-

mical combinations and the manipulations of the laboratory, with which it is no imputation on the physicians to suppose them less familiar than that body who officially preside over pharmacy, and are practically engaged in it. This is an instance in which the dictate of common sense is superseded by adherence to antiquated privilege. A collective institution, such as is proposed, would be far more competent to discharge this important duty. But if such union be deemed inexpedient, provision should at least be made that henceforward the national pharmacopœia should emanate from the apothecaries, the approval of the college of physicians and surgeons being previously obtained.

In discharging the several functions thus hastily sketched, how much more creditably and usefully would medical corporations be employed than in tyrannising over those whom they ought to protect, and grasping, to the sacrifice of every principle of truth and justice, the gains of a sordid self-interest.

It may be convenient ere I close to recapitulate the arrangements of the profession now proposed.

The natural unity of the profession to be explicitly acknowledged, and the qualification of practitioners regulated accordingly.

The practice to be exercised by two classes, physicians and general practitioners, the former practising physic only, the latter physic, surgery, and pharmacy. Druggists are necessary, and some provision should be made to insure their competency as dispensers of medicine. They are in fact what apothecaries were before the wants of society elevated them into medical practitioners. Some medical practice would necessarily devolve even on druggists, however unwilling to undertake it, and no penal enactments could wholly prevent this. It might be safely disregarded under the organization proposed; for so completely would the public necessities be supplied by a well-constituted body of general practitioners accessible to all, that the medical practice of druggists would be no infringement, and need not be even prohibited.

Physicians should be qualified by the universities, a uniform course of education being adopted by all. In framing this regard should be had rather to what experience has proved, than to what speculation may suggest, and special care should be taken not to insist on such high literary attainments as would impair medical competency, or limit the supply of practitioners.

Thus equally qualified physicians should be embodied by an act of incorporation to enrol the members, and guard them from unqualified intrusion. To verify the university degree, and enrol the candidate as a member of the body corporate, is the

only essential function such a corporation or college would have to perform. All members so recognized should have equal privileges, and the offices of the college should be open to all. In such a community talent and information would preserve their just ascendancy; on these alone should claim to advancement be founded; and no privilege should be allotted to any peculiarity of education, or other imputed superiority distinct from personal merit.

In organizing the department of general practice it would be well to render it correspondent to that of the physicians, by separating the primary qualification of practitioners from the superintendence of the practice, and this could now be done with signal advantage by empowering the new London college to grant diplomas in this faculty. This would cause no inconvenience even to young men, who, obliged to attend in London, could, with equal ease, obtain testimonials of qualification from the London university as from the college of Surgeons. This latter body might then restrict itself to the verification of diplomas and the enrolment of members, and to it would apply all the observations already made respecting a college of physicians.

It would be a triumph of sound principle, and conduce to the best interests, both of the profession and the public, if both colleges were united in one comprehensive medical institution, capable in its collective capacity of fulfilling the several contingent functions already specified. An incalculable advantage resulting from such an establishment would be its being made a central depository for collecting from all parts of the empire facts and observations illustrative of medical science, and conducive to its improvement. Such materials abound, but they are too frequently lost for want of communication, or rendered comparatively valueless by too limited circulation. An annual volume issuing from a national medical institution would command general attention, and by embodying the collective experience of the year, digested and authenticated by suitable investigators, would diffuse among the profession much useful knowledge, capable of prompt and salutary application. Questionable facts would thus be quickly confirmed or refuted,—plausible theories be subjected to a philosophic ordeal capable of demonstrating their truth or falsity,—the public be preserved from much delusion,—the profession daily become more enlightened,—medical science advance with a rapidity hitherto unexampled,—and the best interests of mankind be proportionably served. It is difficult to conceive how an organization of the profession, fraught with such manifold and signal advantages as might be realized by that which is now proposed, can fail to

be appreciated, or be suffered to yield to a system possessed of no redeeming quality to compensate the evils with which it abounds.

My former tracts were anonymous, not from any reluctance on my part to avow the opinions which I hold, but from a wish that the facts and conclusions adduced should rest on their own merits alone. A name appeared to me of little consequence. False views or disingenuous reasoning could not derive support from any authority, while I was not without apprehension that, in avowing the author, the cause of truth might suffer from the insignificance of the advocate. Both my feelings and judgment in this respect are unchanged.

I am given to understand, however, by those to whose experience I defer, that anonymous publications on such subjects fail to attract attention, and are relatively valueless; and as I unfeignedly wish the subject matter of the foregoing pages to undergo the scrutiny of the profession, and thereby to direct attention to those efforts which the present exigency requires, I reluctantly comply with the desire conveyed to me, by subscribing to the present essay a name, which, though it cannot promote, will, I trust, not be suffered to injure a cause in which both the profession and the public are so deeply interested.

Bath, August 9, 1827.

EDWARD BARLOW, M. D.

ART. IX.—*On the Effects of the Poisonous Gases on Vegetables.* By EDWARD TURNER, M. D. Lecturer on Chemistry, Edinburgh, &c.; and ROBERT CHRISTISON, M. D. Professor of Medical Jurisprudence and Police in the University of Edinburgh, &c.

HAVING been called on last July to give evidence before the Jury Court, in the case of Hart against Taylor, relative to the effects of a black-ash manufactory on the vegetation in its neighbourhood, and having been unable to find any distinct facts recorded respecting the influence on vegetable life of the gases disengaged in the preparation of that article, we were induced to make some experiments on the subject, which were originally intended for our private satisfaction only. But we have since understood, that the case in which we were concerned is no less than the third or fourth instance in Scotland of the same kind of manufactory having subjected the proprietors to a prosecution on the ground of its destructiveness to vegetation, while nevertheless no data have hitherto been

made public, except those derived from the very equivocal and contradictory testimony of unscientific witnesses as to the actual mischief done around the works. We have therefore conceived that a succinct account of our observations may be useful to others who may chance to be similarly circumstanced with ourselves.—Since the trial, besides repeating our original experiments, we have tried the effects of other gases; and we have annexed the results, not that we imagine them to be important in themselves, but for the purpose of drawing the attention of the botanist to this little tried, but probably valuable instrument for investigating the physiology of plants. The interesting experiments of M. Marcet on the effects of solid and liquid poisons on vegetable life,* and those related in the subsequent remarks regarding the effects of the gases, will show, that the operation of poisons may furnish the attentive inquirer with as many useful facts in the case of vegetable as in that of animal physiology.

It was at first our intention to give an account of the leading facts brought out upon the several jury-trials alluded to. But on inquiry, we have found the evidence, as in most prosecutions for this description of nuisance, so opposite and irreconcilable, that we must rest contented with an abstract of our personal researches. The black-ash, we may simply observe, has been for many years a most important and extensive article of manufacture, being one of the substances formed in the course of the process for converting into soda the Glauber's salt, and more particularly the refuse in the making of bleaching powder. In the course of the process for preparing it, some chlorine and a good deal of sulphurous acid are evolved, and in those manufactories in which the black-ash is converted into carbonate of soda, an additional quantity of sulphurous acid is disengaged. It was to these gases, therefore, and more especially to the sulphurous acid, that our attention was drawn in the first instance.

The *Sulphurous acid gas* appears to be exceedingly deleterious to vegetables, even in very minute quantity and proportion. We first observed that when four, or even only two cubic inches were introduced, along with a young mignonette plant, into the air of a glass jar of the capacity of 470 cubic inches, the leaves of the plant became greenish-gray, and drooped much in less than two hours and a half; and though then taken out and watered, it soon died altogether. We next found that when somewhat less than half a cubic inch was introduced into a jar of the capacity of 509 cubic inches, a mignonette plant introduced along with it began to lose its colour and to droop

* Ann. de Chim. et de Phys. xxix. 200.

in three hours; and although taken out three hours after that it languished, and gradually died in the course of a few days.

But the peculiar effects of the gas, and its extraordinary destructiveness, are better shown when its quantity and proportion to the air are still less,—as, for example, in the following experiment. A wide-mouthed bottle, containing a mixture of six cubic inches of air and $\frac{2.3}{100}$ ths of a cubic inch of sulphurous acid, was fixed, mouth uppermost, on a stand twenty inches high;—at the bottom stood a young mignonette plant, a young laburnum tree six inches tall, and a young larch, which had been all transplanted at least five days before, and had recently been well watered;—and over the whole was placed and carefully luted to the table a glass jar two feet high, and of the capacity of 2000 cubic inches, so that the proportion of sulphurous acid was nearly a 9000th part. The jar stood in a situation where it was exposed to a bright diffused light, but not to the sunshine. We had previously found in a similar experiment with a mignonette plant alone, that it became affected in less than twenty-four hours; and in a subsequent experiment with a laburnum visible effects were caused by the same quantity and proportion in nine hours. In the present instance, on account of the large surface of moist earth exposed, which would absorb some of the gas, the effect was slower and less complete. In forty-eight hours, however, all the plants were distinctly acted on. The tips of some of the mignonette leaves and larch leaves were grayish-coloured, shrivelled, and dry-looking, and the three lower leaves of the laburnum were speckled with irregular grayish-yellow dry-looking spots. The air was then renewed, together with the same proportion of gas, and it was allowed to act for forty-eight hours longer, during which the effects slowly extended towards the footstalks and over more of the leaves. The plants were then taken out. The earth continued moist. The middle leaves of the mignonette, but the lower leaves of both the larch and the laburnum had suffered most. The leaves of the mignonette were withered chiefly at the tips or edges;—most of those of the larch at the tips, in some, however, at the middle;—those of the laburnum were affected more uniformly over their whole disk, they drooped considerably, a gentle touch was sufficient to break the leaflets from the footstalks, and the footstalks from the stem,—nay, some of the leaflets had dropped off while the plant was in the jar. The bud-leaves were not injured in any of the plants.

The effect of this poison seemed to resemble considerably the ordinary decay of the leaves in autumn. It was invariably produced in several repetitions of the foregoing experiment. The

proportion of gas was sometimes a ten-thousandth only, the quantity a fifth of a cubic inch, and yet the whole unfolded leaves were nearly destroyed in forty-eight hours. We remarked that the withering and curling of the leaves always rapidly increased for an hour or two after the removal of the plants into the open air, so that some leaves which had been apparently but little affected in the jar died afterwards in the air quickly and completely. The whole plant, however, was never killed by these small proportions. Segments near the foot-stalks, particularly of the upper leaves, continued green and juicy, and the buds put forth fresh though generally stunted leaves. Even when the proportion of gas was larger, the plants were not killed altogether: the stem was not affected unless the proportion was considerable, and even then it was only the top which suffered.—It will be afterwards seen how different these phenomena are from the effects of deleterious gases not irritants.

It is particularly worthy of notice that the proportion of sulphurous acid which thus seems to be so deadly to plants, is hardly or not at all discoverable by the smell. When a ten-thousandth part of sulphurous acid gas was intimately mixed with air, we could not be confident that its odour was perceptible, although we have reason to think our sense of smell is acute; and when this proportion was mixed in the same jar, and in the same manner as in the experiments related above, but without either plant or moisture being present, we were certain that in eight hours no part of the air in the jar had the slightest odour of the gas. This fact, though simple, is an important one in regard to what has been stated in evidence on some prosecutions against chemical manufactories,—namely, that the emanations were not perceptible to the smell in the neighbourhood of the works, and therefore not likely to be in a state sufficiently concentrated to prove injurious.

We do not pretend to infer, as an absolute conclusion from these experiments, that manufactories from which sulphurous acid gas is disengaged even in large quantities must be injurious to surrounding vegetation. It is probable, nay certain, that, small as the proportion was which we have tried, the proportion contained in the air in the vicinity of the largest manufactories is a great deal less. But it seems certainly a fair presumption, that, when such extraordinary effects are produced in so short a time, by so minute a quantity, and notwithstanding so liberal an admixture of air, effects analogous, if not quite so fatal, will be produced by the lengthened application of a larger quantity, though inferior proportion of the same poison, in a state of constant renewal.

The next gas, whose effects we have particularly examined, is the *Hydrochloric*, or *Muriatic acid gas*.

Its effects have appeared to us not inferior, nay even superior to those of the sulphurous acid. In our first experiments with it we found, that four cubic inches and a half mixed with a hundred times their volume of air, and two cubic inches mixed with four hundred volumes, began to turn the leaves of a mignonette plant grayish-yellow in ten minutes, then caused it to droop, and in five hours killed it altogether and irrecoverably; and that the leaves were wet and acid. We next remarked that half a cubic inch in a thousand times its volume of air began to affect another mignonette plant in an hour, and killed all the leaves in twenty hours; that the plant, though then taken out with its stem erect, soon died; but that the leaves were not damp or acid, as in the former instances. Various inferior proportions were tried with similar results, till at length the following experiment was made:

A fifth part of a cubic inch of hydrochloric acid gas was mixed with three inches of air in a long jar, and placed under a glass receiver of the same size and form, and with the same precautions to secure a gradual and thorough mixture, as in the experiments with the sulphurous acid: The plants chosen were a healthy laburnum tree five inches tall, and a little larch tree. In nine hours the edges of some of the laburnum leaves appeared to be curling inwards; in twelve hours this effect was unequivocal: As the latter examination was made by candle light, we could not say positively whether the colour had faded. In twenty-four hours the leaves had all acquired a dull grayish-green colour, and dry appearance, and their edges were crisped and curled. The crisping effect increased during the next twenty-four hours, after which the plants were removed into the open air of the room. The lower leaves were evidently most affected, and some of them even dropped their leaflets when the plant was shaken; none, however, had escaped, except a young half-unfolded one at the top; all the rest seemed quite dead. As in the case of the sulphurous acid, so here the crisping and curling of the leaves increased rapidly after the removal of the plant from the jar; an effect which is probably to be ascribed to the more rapid evaporation of their moisture, no longer renewed from the leaf-stalks. The larch plant, whose changes could not be properly seen in the jar, on account of its colour being nearly the same in its natural as in its withered state, was found on close examination to be wrinkled and dry, particularly on the lower leaves, and on the tips of them. Neither of these plants, however, died; the unfolded leaves were all destroyed, but the buds gradually expanded themselves, though nipped on the tops.

This gas must therefore be very injurious to vegetable life, since so small a quantity as a fifth of an inch, although diluted with 10,000 parts of air, destroyed the whole vegetation of a plant of considerable size in less than two days. Nay, we afterwards found that a *tenth part of a cubic inch in 20,000 volumes of air* had nearly the same effects. In twenty-four hours the leaves of a laburnum were all curled in on the edges, dry, and discoloured; and though it was then removed into the air, they gradually shrivelled and died. Like the sulphurous acid, the hydrochloric acid gas acts thus injuriously in a proportion which is not perceptible to the smell. Even a thousandth part of hydrochloric acid gas is not distinctly perceptible; a ten thousandth made no impression on the nostrils whatever, although great care was taken to dry thoroughly the vessels used in making the mixtures.

The other gases whose effects we have examined are chlorine, nitrous acid gas, sulphuretted hydrogen, ammonia, cyanogen, carbonic oxide, olefiant gas, and protoxide of azote. But our examination of these has been cursory.

Chlorine may be expected to have the effects of hydrochloric acid gas; and so indeed it has; but they appear to be developed more slowly. Two cubic inches in two hundred parts of air did not begin to affect a mignonette plant for three hours; half a cubic inch in a thousand parts of air did not injure another in twenty-four hours; but when the plants did become affected, the same drooping, bleaching, and desiccation were observed.

Nitrous acid gas is probably as deleterious as the sulphurous and hydrochloric acid gases. In the proportion of a hundred and eightieth it attacked the leaves of a mignonette plant in ten minutes; and half a cubic inch in 700 volumes of air caused a yellowish green discoloration in an hour, and drooping and withering in the course of twenty-four hours. The leaves were not acid on their surface.

The effects of *Sulphuretted-hydrogen* are quite different from those of the acid gases. The latter attacked the leaves at the tips first, and gradually extended their operation towards the leaf-stalks; when in considerable proportion their effects began in a few minutes, and if the quantity was not great the parts not attacked generally survived if the plants were removed into the air. The sulphuretted hydrogen acts differently. Two cubic inches in 230 times their volume of air had no effect in twenty-four hours. Four inches and a half in eighty volumes of air caused no injury in twelve hours, but in twenty-four hours several of the leaves, without being injured in colour, were hanging down perpendicularly from the leaf-stalks and quite flaccid; and though the plant was then removed into the open air the

stem itself soon began also to droop and bend, and the whole plant speedily fell over and died. When the effects of a large quantity, such as six inches in sixty times their volume, were carefully watched, it was remarked that the drooping began in ten hours at once from the leaf-stalks, and the leaves themselves, except that they were flaccid, did not look unhealthy. Not one plant recovered, any of whose leaves had drooped before it was removed into the air.

The effects of *Ammonia* were precisely similar to those of sulphuretted hydrogen just related, except that after the leaves drooped they became also somewhat shrivelled. The progressive flaccidity of the leaves,—the bending of them at their point of junction with the foot-stalk, and the subsequent bending of the stem,—the creeping, as it were, of the languor and exhaustion from leaf to leaf, and then down the stem, were very striking. Two inches of gas in 230 volumes of air began to operate in ten hours. A larger quantity and proportion seemed to operate more slowly.

These phenomena, when compared with what was observed in the instances of sulphurous and hydrochloric acid, would appear to establish, in relation to vegetable life, a distinction among the poisonous gases nearly equivalent to the difference existing between the effects of the Irritant and the Narcotic poisons on animals. The gases which rank as irritants in relation to animals seem to act locally on vegetables, destroying first the parts least plentifully supplied with moisture. The narcotic gases—including under that term those which act on the nervous system of animals, destroy vegetable life by attacking it throughout the whole plant at once. The former probably act by abstracting the moisture of the leaves, the latter by some unknown influence on their vitality. The former seem to have upon vegetables none of that sympathetic influence upon general life, which in animals follows so remarkably injuries inflicted by the purest local irritants.*

* Other occupations prevent us making the requisite inquiries for examining such practical conclusions as our experiments seem to lead to. One, however, may be briefly mentioned. The differences described above show that the injury sustained by vegetation in the heart and vicinity of a great city must be caused by some irritant gas, if circumstances regarding the soil and exposure do not account for it, which they seldom or never will. The plants are rarely killed altogether; their vegetation is merely blighted for the season. In the spring it recommences with a luxuriance not surpassed in country situations; but when the leaves attain maturity, and have no longer the superabundant supply of moisture they received while young, they are blighted anew. How exactly do these phenomena concur with what we should expect from the influence of the irritating gases as ascertained in our experiments. Such a gas, it is well known, is disengaged in abundance from our coal-fires,—namely the sulphurous acid. There has long been a vulgar and very general notion, that coal smoke diffused and diluted in the atmosphere hurts vegetation; but we are not aware that it has rested till now on any solid reason. A curious fact, indeed, and the only one we have been able to find, is mentioned in Evelyn's *Fumifugium*,—namely, that one year, when Newcastle was besieged during the civil war at the Revolution, and coals were in consequence exceedingly scarce in London, in various

Cyanogen appears allied to the two last gases in property, but is more energetic. Two cubic inches diluted with 230 times their volume of air affected a mignonette plant in five hours; half a cubic inch in 700 volumes of air affected another in twelve hours; and a third of a cubic inch in 1700 volumes of air, affected another in twenty-four hours. The leaves drooped from the stem without losing colour, and removal into the air, after the drooping began, did not save the plants.

Carbonic oxide is also probably of the same class, but its power is much inferior. Four cubic inches and a half diluted with 100 times their volume of air had no effect in twenty-four hours on a mignonette plant. Twenty-three cubic inches, with five times their volume of air, appeared to have as little effect in the same time; but the plant began to droop when it was removed from the jar, and could not be revived.

Olefiant gas in the quantity of four cubic inches and a half, and in the proportion of a hundredth part of the air, had no effect whatever in twenty-four hours.

The *Protoxide of nitrogen*, or intoxicating gas, the last we shall mention, is the least injurious of all those we have tried; indeed it appears hardly to injure vegetation at all. Seventy-two cubic inches were placed with a mignonette plant in a jar of the capacity of 509 cubic inches for forty-eight hours; but no perceptible change had taken place at the end of that time.

In all of the foregoing experiments, with the exception of those made with minute proportions of the sulphurous and hydrochloric acid gases, the gas was placed at the bottom of the jars, and the plant on a stage raised nearly to the top of them. It is hardly necessary to add, that comparative experiments were also made on plants in jars of pure air.

It is well known that plants are sometimes affected differently by the same agent. Some thrive in situations which are injurious to others, and where to all appearance the difference does not depend on the nature of the soil alone. Sir H. Davy found that while some plants grow in an atmosphere of hydrogen, others are speedily destroyed by that gas. We may hence anticipate that a variety of curious and interesting results would be obtained by experiments directed with this view; but circumstances have not permitted us to undertake the inquiry. We may observe, however, that of six different species of plants subjected to the action of sulphurous acid, all of them appeared to be affected by that gas nearly in an equal degree.

gardens of the nobility in the metropolis fruit trees, which never bore fruit either before or after, were loaded with it. But the *Fumifugium* contains so many monstrous and incredible facts, and shows such an uncompromising determination to call in the aid of coal-smoke to account for everything wrong in London which could not be more easily explained, that the modern inquirer will naturally pause before he gives the lively and ingenious author credit even for what appears probable.

PART II.

CRITICAL ANALYSIS.

ART. I.—*Recherches Physiologiques, &c.* Physiological and Chemical Inquiries on Digestion. By MM. LEURET and LASSAIGNE. Paris, 1825.

Die Verdauung nach Versuchen, &c. Experiments on Digestion. By Professors GMELIN and TIEDEMANN of Heidelberg. 1825. Part I.

Recherches, &c. The same Work translated into French. By M. JOURDAN. Paris, Part I. 1826, Part II. 1827.

(*Continued from last Volume, p. 425.*)

WE shall now proceed with the analysis of these works, commencing with the observations which have been made on the *Muscular Action and Secretions of the Stomach*.

The Heidelberg Professors have not thought it necessary to make any personal researches regarding the muscular action of the stomach, but have simply adopted the conclusions originally drawn by Philip, Brodie, and Broughton, and more recently modified by the experiments of MM. Breschet and Mylne Edwards. According to the results obtained by the two last physiologists, the muscular coat of the stomach, under the influence of the pneumogastric nerves, pushes forward towards the pylorus the portions of food which successively undergo chymification in contact with the villous membrane;—the effect of the division of the pneumogastric nerves in retarding digestion is owing merely to the muscular motions being suspended, by which the outer layers of the food are propelled;—and the usual rapidity of digestion is restored by any suitable irritation applied to the ventricular ends of the divided nerves, more particularly, however, by galvanism.

But MM. Leuret and Lassaigne have resumed and varied the experiment of the division of the pneumogastric nerves;

and they state as the result of their inquiries, that the only obvious and necessary effect of the operation is to paralyze the sphincter muscle of the cardiac orifice of the stomach, and that the retardation of digestion is simply owing to the food being expelled into the œsophagus by the action of the stomach. For they found that when five or six inches of each pneumogastric nerve were cut away in the neck of a horse, and the gullet was tied after the animal was fed with oats, digestion went on as rapidly as usual : In eight hours one-half of the oats had passed the pylorus, what remained in the stomach was all converted into chyme, and the lacteals were everywhere distended with a white fluid possessing the physical and chemical properties of chyle. This experiment was frequently repeated, and invariably with similar results.

Professors Tiedemann and Gmelin conceive, from a few experiments on dogs, that the division of the pneumogastric nerves also alters the chemical constitution of the gastric juice, namely, by depriving it of its acidity. This appears too important a fact to be decided by the few trials they have made ; and in the meantime it is rendered doubtful by the perfect mode in which, under the same circumstances, digestion was found by the Parisian physiologists to be carried on.

The *Gastric Juice*, which has been made so frequently the subject of inquiry, and with such discordant results, has been examined anew both by Leuret and Lassaigue, and by Tiedemann and Gmelin ; and our information as to its properties and agency in digestion has in consequence been rendered much more precise. Under this head the most extensive and valuable researches unquestionably appear to be those of Tiedemann and Gmelin.

MM. Leuret and Lassaigue allege they have invariably found the gastric juice to be acid ; they state its component parts to be water, hydrochlorate of ammonia, chloride of sodium, mucus, an animal principle soluble in water, phosphate of lime, *lactic acid* ; and they impugn the accuracy of the experiments of Dr Prout, who, it is well known, was lately led to infer that the free acid evolved during digestion is the hydrochloric.

The Heidelberg physiologists, on the contrary, inform us, that if the contents of the stomach be examined after a long fast, and without any stimulus being applied to its villous membrane, the fluid found in it is a clear, ropy, faintly-opaque liquid, which is almost or entirely destitute of acidity. But if any stimulus, even of the simplest kind, is applied to the inside of the stomach, then the fluid secreted is uniformly acid,

and the degree of acidity appears to be proportional to the degree of the stimulus. Pure gastric juice they found to be best procured by making animals swallow quartz pebbles after a long fast, and killing them in an hour. It was generally grayish-white, ropy, and decidedly acid. It contained, as procured from the dog and the horse, some mucus, osmazome, and salivary matter, alkaline sulphates and hydrochlorates, the alkali being chiefly soda, phosphate and muriate of lime, other salts in minute proportion; and the acidity was owing to the *hydrochloric* and *acetic acids* in the dog, and to these conjoined with the *butyric acid* in the horse. That part of their chemical inquiries which regards the free acids seems to have been conducted with great care; and certain criticisms of Leuret and Lassaigne have given occasion to an appendix, consisting of a defence of themselves and of Dr Prout, which we cannot help thinking is quite satisfactory. As the lactic acid of Leuret and Lassaigne is now acknowledged by Berzelius to be a variety of the acetic, all parties may be understood to be agreed as to the existence of that acid in the gastric juice. The researches of Prout, Children, and Graves, confirmed as they have been so amply by Tiedemann and Gmelin, likewise establish the presence of the hydrochloric acid. The existence of the butyric is more doubtful.

When the secretion of the gastric juice is elicited by its natural stimulus, food of various kinds, it has been remarked by Leuret and Lassaigne, as well as by Tiedemann and Gmelin, that the chymous mass is invariably acid, and that its acidity is owing to the acids mentioned above; and Tiedemann and Gmelin farther maintain as the result of their experiments, that its acidity is greatest when the food is most difficult of digestion. In dogs and cats the greatest acidity was remarked when they were fed with coagulated albumen, fibrin, bones, gristle; it was less when they were fed on starch, gelatin, potatoes, rice; and when they were fed with liquid albumen, the alkalinity of the food was nearly sufficient to neutralize the acidity of the gastric juice. The quantity secreted during digestion seems to be very great; Leuret and Lassaigne having found, that when the gullet of a horse was tied so as to prevent the secretions of the mouth and gullet from entering the stomach, a full meal of oats became completely saturated with juice in four or five hours.

So much with regard to the nature and composition of this fluid. Its power of dissolving the food has likewise been made the subject of experiment by the Parisian and by the Heidelberg physiologists; and they agree in confirming the statements of Spallanzani, Stevens, Gosse, and others, and in contradicting the

later results of Montégre, who imagined he found that the gastric juice does not act out of the body, and whose opinion has since regulated that of the French school generally. MM. Leuret and Lassaigne remarked that the fluid procured by long sponges from the stomach of a duck while fasting, when kept upon bread crumbs at a temperature of 88° Fahr. soon divided them into minute particles, and formed with them a homogeneous mass precisely like chyme; and that when flesh was mixed with the gastric juice of the dog, it was quickly softened and lost weight. The observations of Tiedemann and Gmelin are more detailed. The fluid found in the stomach of the dog during the digestion of bones and coagulated albumen was made the subject of experiment, and comparative observations were made with water and with milk. Various kinds of food were tried, such as bread, coagulated albumen, raw flesh, boiled flesh; and in every instance it was observed that the bread was broken down in the course of eight or ten hours into a pap, and the surface of the beef and of the albumen was converted into a pulp, which could be easily scraped off. It is evident that Montégre failed in procuring similar results, because the fluid with which he operated was not gastric juice, secreted in consequence of the application of some stimulus to the stomach, but a mixture of saliva, the mucus of the gullet and the fluid which Tiedemann and Gmelin found in the stomach while empty of food and not stimulated.

The solvent power of the gastric juice, therefore, is now placed beyond all question. It is only the German experimentalists who have thought of trying whether this property is accounted for by the composition of the fluid; and of attempting to accomplish, by means of the simple substances contained in the gastric juice, the same solution or digestion which is effected by the gastric juice itself. Their experiments, however, are not complete. They have remarked, that dilute acetic acid, dilute hydrochloric acid, a weak solution of acetate of ammonia, and a solution of hydrochlorate of ammonia, severally dissolve more or less of most animal substances which are used as articles of food. They have not tried their effects conjunctly as they exist in the gastric juice.

Although these experiments, however, are not satisfactory, no doubt can exist that the gastric juice possesses the power, as a mere chemical agent, of effecting the solution of a great variety of kinds of food. Whether this chemical property is, with the aid of the churning action of the stomach, sufficient for effecting the process of chymification remains now to be considered. The mucous crypts of the stomach evidently pour out an abundant mucus; but it is probable that this only serves for lubri-

cating the stomach and food, and perhaps for diluting it. It is impossible, indeed, to collect the mucus of the stomach, so as to ascertain whether or not it possesses also the power of dissolving vegetable and animal matter; but this certainly appears improbable. The bile has no share, as has been imagined by some physiologists, in converting the food into chyme. According to the late experiments of Brodie and Mayo, and those presently to be mentioned of Leuret and Lassaigue, and of Tiedemann and Gmelin, chymification goes on perfectly in animals fed after the biliary duct has been tied. There is no other apparent agent, therefore, in the process, except the gastric juice and the muscular action of the stomach. It appears to us, however, that the question should have been set at rest by subjecting the food during artificial digestion to a process of churning, equivalent to that to which it is subjected by the stomach, and then comparing the chemical changes which take place with those produced on the principles contained in the food after they have been subjected to natural chymification.

The changes produced by natural chymification have received a little of the regard of MM. Leuret and Lassaigue, and have been studied with great care by Professors Tiedemann and Gmelin. It is not easy to present in moderate compass an outline of the results which they have obtained; but the following summary will be found to contain all that is most material. The animals chosen for these experiments were dogs and horses. Liquid albumen forms under the natural process of digestion a homogeneous fluid, in which the albumen remains quite unaltered; and this sort of chyme passes the pylorus more rapidly than any other. Coagulated albumen is much more slowly dissolved, and the fluid formed possesses the properties of coagulated albumen dissolved in acetic acid. Fibrin and vegetable gluten undergo a similar change. Gelatin is converted into a clear brownish fluid, in which neither gelatin nor albumen can be discovered. White cheese forms an opaque, dirty-white fluid, containing much animal matter, which, however, is neither casein, gelatin, nor albumen. Starch is gradually dissolved, and loses its reaction with iodine, being converted into sugar and amidine. The results obtained with compound articles of food, such as milk, beef, bread, and oats, in various states of mixture, were such as the foregoing facts would lead one to anticipate. These need not be particularized here. Bones gave a liquid which contained not only animal matter, but likewise a large quantity of lime. The general result of the whole experiments is, that all the animal principles, except liquid albumen, undergo a material change during their solution in the gastric juice, and that the

change generally consists in their being made to approach nearer in nature to albumen. The statement of Dr Prout, that albumen is never found in the stomach, or higher up than the duodenum, appears to be an error. It is to be regretted that but little attention has been paid by both the Parisian and the Heidelberg experimentalists to the mode in which unazotized vegetable substances are animalized, which appears one of the most important and least understood of all the changes effected by the process of digestion. The experiments before us are unsatisfactory, and in some respects even contradictory. For example, it does not seem to be settled that fecula is animalized at all in the stomach; for, contrary to what has been stated on the authority of Gmelin and Tiedemann, it has been found by Leuret and Lassaigne to be transmitted through the whole alimentary canal unchanged, and not to form any chyle, unless it has been mixed with azotized food. According to the latter, wine and spirituous liquors are acidified during the progress of digestion.

During the chemical changes which digestion effects on the food various gases are disengaged. Chevreul found the gas in the stomach to be composed of carbonic acid, oxygen, azote, and hydrogen. Leuret and Lassaigne have found it to consist, as taken from the dog's stomach, of carbonic acid forty-three per cent. azote thirty-one, carburetted hydrogen twenty, oxygen four, sulphuretted hydrogen two. Tiedemann and Gmelin have also found the last gas in the ox and in the sheep.

The next subject which comes under notice is the change which the chyme undergoes in the intestines. Here it is necessary to set out with an account of the nature and uses of the various fluids which are added to the chyme after it has passed the pylorus. These are the bile, pancreatic juice, and secretion of the intestinal mucous membrane.

We have first then to consider the constitution and uses of the *Bile*. In discussing this subject MM. Leuret and Lassaigne begin by adopting very nearly the analysis made some time ago by Thenard; according to whom it contains resin picromel, and yellow colouring matter as its organic ingredients, and the following salts, phosphate, muriate and sulphate of soda, muriate of potass, phosphates of lime and magnesia, free soda, and a little iron. Since this analysis, cholesterine, the chief principle in biliary calculi, has been detected in bile by Chevreul, its discoverer.

But Professors Tiedemann and Gmelin have analyzed it anew with great care; and the result has been, that many other organic principles have been discovered in it, and that it proves to

be the most complex of all the animal fluids. They agree very nearly with Thenard as to the saline ingredients, and also with Chevreul as to the presence of cholesterine, which they appear to have detected about the same time as that chemist, and without being aware of his discovery. But they have found the picromel of Thenard to be a compound substance, consisting of resin of bile and a crystalline principle, which has a very sweet and faintly bitter taste, like liquorice, possesses all the properties of sugar, except that of fermenting, contains azote, resembles very exactly the sugar of gelatin procured by Braconnot on treating isinglass with sulphuric acid,—and is one of the constituents of the bile which has the property of rendering the resin soluble in water. They have allowed it to retain the name of *picromel*. Sugar of bile would have been a more appropriate term.

The next addition they have made to the analysis of the bile is the discovery of a principle resembling the asparagin found by Vauquelin and Robiquet in the asparagus. When pure it is in the form of colourless crystals, which are six-sided prisms, terminated by four or six-sided pyramids. They are soluble in about sixteen parts of temperate water, insoluble in pure alcohol, very soluble in nitric acid, but without undergoing any decomposition, fusible, and they have a peculiar fresh taste. The *Biliary asparagin*, as they have called it, is another substance, which, like picromel, renders the resin soluble in water.

Besides the principles already mentioned, Tiedemann and Gmelin have also found in the bile the mucus of the gall-bladder, casein, albumen, gliadine, or that part of gluten which is soluble in alcohol, a little osmazome, oleic acid, margaric acid, acetic acid, and a new acid, the *cholic acid*. They have likewise found that the soda contained in it exists in the state of bicarbonate, that there is also a little ammonia, and that there is no sulphuretted hydrogen in it, as Vogel has supposed. Lastly, they infer that it contains a peculiar colouring matter, because re-agents act on bile in the same way as on the colouring principle of biliary calculi; but they have not been able to separate it.

The *Cholic acid* is procured from the alcoholic extract of bile in the form of acicular crystals. It is distinguished from other acids by a sweet taste. In its properties it resembles the uric, pyro-uric and purpuric acids, but it has a stronger affinity for bases.

On the effects of re-agents on the colouring principle of the bile they have made some interesting observations, from which we shall select, as furnishing the best practical test of the presence of bile, the changes caused by nitric acid. When nitric

acid is added in small successive portions to bile of every kind, it causes first a green, then blue, next violet, and lastly a red coloration, which becomes yellow on standing, or on the addition of a large excess of acid.

The bile therefore appears to be an exceedingly complicated fluid. Besides saline ingredients of the inorganic class, it contains mucus, albumen, osmazome, gliadine, casein, picromel, asparagin, acetic acid, oleic acid, margaric acid, cholic acid, resin, and colouring matter.

We shall next proceed to consider what has been done by the rival experimentalists to determine the long agitated question of the *Uses of the bile*. A few years ago Mr Brodie was led to infer, from some experiments on the effects of tying the excretory duct of the liver in animals, that the chief purpose of the bile is to separate the chyle from the chyme. For he remarked that when the *choledochus* duct was secured with a ligature, and food then given, chymification went on in the stomach as usual, but no chyle could be found in the intestines, or in the lacteals. The lacteals contained a transparent fluid, which he supposed to be lymph, and the watery part of the chyme.* Mr Brodie's observations have been confirmed by the subsequent experiments of Mr Herbert Mayo, who remarks, that when the *choledochus* duct was tied in the cat or dog, and the animal killed at various intervals after eating, "there was no trace whatever of chyle in the lacteal vessels."†

These results are at variance with the experiments both of Leuret and Lassaigue, and of Gmelin and Tiedemann. The former tied the common duct in a dog, and cleared out the intestines by giving it a little castor oil. Twelve hours after the operation they gave it bread and milk with sugar thrice at intervals of six hours; and eight hours after the last meal it was strangled, and examined immediately. The stomach contained an acid pulp; a very soft, whitish, sweet-tasted chyme adhered to the villous coat of the duodenum; this matter increased in consistence downwards; and in the great intestines it was firm, but had the same colour, and was nearly destitute of taste or smell. The thoracic duct was distended with a yellowish red, transparent fluid, which coagulated on standing exposed to the air, and yielded the usual proportion of fibrin, albumen, and saline matters.

The experiments of Tiedemann and Gmelin are much more elaborate and precise. They remarked that the animals were attacked with vomiting soon after the operation, then with thirst

* Journal of Science and the Arts, xiv. 343.

† Medical and Physical Journal for October 1826.

and aversion to food ; on the second or third day the conjunctiva of the eyes became yellow ; the stools chalky and very fetid, and the urine yellow and convertible to blue, and then to red, with nitric acid. Some of the animals died ; some were killed. Of the latter some had previously recovered from the jaundice, which was owing to a singular phenomenon also noticed by Mr Brodie in his experiments, the re-establishment of the duct by the effusion of lymph around the tied part, and the subsequent dropping of the ligature. In those in which the biliary duct continued impervious the colouring matter of the bile was found in the blood, the serous membranes, the cellular tissue, the coats of the arteries and veins, and in the fat. Like Brodie, Mayo, and MM. Leuret and Lassaigne, they observed that chymification went on as perfectly as in a sound animal. In the small intestines they found nearly the same principles as in sound animals, with the exception of those derived from the bile ; and in particular they found in the duodenum, and in contact with its membrane, the soft mucous flakes which some physiologists consider, but as our authors imagine erroneously, to be chyle. The contents of the great intestines were likewise, with the exception of the absence of certain biliary principles, the same as in sound animals, but they had an exceedingly fetid and disagreeable odour. The thoracic duct, and the lacteals in animals fed recently before death, always contained an abundant fluid, which was generally of a yellowish colour. It coagulated, like ordinary chyle ; the crassamentum acquired the usual red colour ; in short, the only difference between it and the chyle seen in a sound animal was, that after the tying of the choledochus duct it was never white. The reason of the difference appears to be, that the white colour is owing to fatty matter, taken up from the food by means of the bile, which possesses the power of dissolving fat, and probably, therefore, aids in effecting its solution in the chyle at the mouths of the lacteals. Mr Brodie appears to have been misled by the absence of the white colour, which the chyle usually possesses, but which it is well known equally to want in ordinary digestion, if the food does not contain fatty matter. Professors Tiedemann and Gmelin confine the agency of the bile in chylification simply to the accomplishing the solution of the fatty matter.

The question, therefore, comes to be, what are really the purposes served by the bile ? This question has been fully considered by the Heidelberg professors. They conceive, in the first place, that by its stimulant properties it excites the flow of the intestinal fluids, which is clearly proved to be the case by the unusual dryness of the feces in jaundiced persons, and in animals whose duct has been tied. In the next place, it pro-

bably stimulates the intestinal muscles to action. In the third place, considering the abundance of highly azotized principles it contains, it probably contributes to animalize those articles of food which do not contain azote. Fourthly, they believe it tends to prevent the putrefaction of the food during its course through the intestines, because when it is prevented from flowing into them their contents appear much farther advanced in decay than in the healthy state. Fifthly, as already mentioned, it probably tends to liquefy and render soluble the fatty part of the food. But, lastly, they are disposed to consider it also as an important excretion.

The arguments by which they endeavour to support this opinion, and more particularly to prove that it is supplementary to the function of the lungs, are ingenious, if not conclusive. They first show, from the relative size of the *vena portæ* and hepatic ducts, from the more intimate connexion of the biliary capillaries with those of the *vena portæ*, than with those of the hepatic artery; and, finally, from the experiments of Malpighi, recently confirmed by those of Simon,—that the bile is a secretion from venous, not from arterial blood. They next prove that a great number of the principles of the bile, such as its resin, colouring matter, fatty matter, mucus, and salts, are thrown out of the body with the feces, in the natural state of the biliary system, or by the urine, and into the cellular tissue, when the excretory duct of the liver is obstructed. These principles all contain a large proportion of carbon, and would appear, therefore, to be intended to carry off the excess of that element which is introduced into the system with the vegetable part of the food, and which is not thrown off by the lungs. In the lungs it is thrown off in a state of oxidation; in the liver it is thrown off chiefly in union with hydrogen, as in the form of resin and fatty matter. That the bile is thus intended to assist the lungs in decarbonizing the blood appears, they conceive, from the following facts. The resin of the bile abounds most in herbivorous animals, whose food contains a great disproportion of carbon and hydrogen. But, what is of more importance, the pulmonary and biliary organs are in different tribes of animals, nay, even in different individuals of the same species, in a state of *antagonism* to one another. The size of the liver and the quantity of the bile are not proportionate to the quantity of food and frequency of eating; but inversely proportional to the size and perfection of the lungs. Thus, in those warm-blooded animals, which have large lungs, and live always in the air, the liver, compared with the body, is proportionally less than in those which live partly in water. The liver is proportionally still larger in reptiles which have lungs

with large cells, incapable of rapidly decarbonizing the blood,—and in fishes, which decarbonize the blood but slowly by the gills,—and above all, in molluscous animals, which effect the same change very slowly either by gills or by small imperfectly developed lungs. It is also worthy of remark, that the quantity of venous blood sent through the liver increases as the pulmonary system becomes less perfect. In the mammalia and in birds the *vena portæ* is formed by the veins of the stomach, intestines, spleen, and pancreas; in the tortoise it receives also the veins of the hind-legs, pelvis, tail, and the *vena azygos*; in serpents it receives the right renal, and all the intercostal veins; in fishes it receives the renal veins, and those of the tail and genital organs. Farther, during the hybernation of certain animals of the class mammalia, when the respiration is suspended, and no food is taken, the secretion of bile goes on. Another argument is drawn from the physiology of the fœtus, in which the liver is proportionally a great deal larger than in the adult, and in which the bile is secreted abundantly, as appears from the great increase of the meconium during the latter months of utero-gestation. The last argument is drawn from pathological facts. In pneumonia and phthisis the secretion of the bile, according to the observations of our authors, is increased; in diseases of the heart the liver is enlarged, and in the *morbus caruleus* the liver retains its fœtal state of disproportion. In hot climates, too, where, in consequence of the greater rarefaction of the air, respiration is less perfectly carried on than in colder climates, a vicarious decarbonization is established by an increased flow of the bile.

The next fluid whose nature should now be considered is the *Pancreatic Juice*. This, however, has been already discussed. We have already mentioned that Leuret and Lassaigne differ materially from Tiedemann and Gmelin as to the composition of the pancreatic fluid. On the whole, we are disposed to adopt the researches of the latter gentlemen,—who find that it contains a very large quantity of highly azotized principles, namely, albumen, cascin, osmazome, and a matter which is turned red by chlorine,—and who believe that one of its uses is to aid in animalizing such vegetable food as does not contain azote.

Besides the bile and pancreatic juice, the chyme receives in the intestines an additional admixture of *Mucus*, or other secretions from the intestinal villous membrane. Tiedemann and Gmelin remarked, that in animals which had fasted long, the inner surface of the small intestines was covered with a thin layer of firm mucus, tinged faintly yellow with the bile, and

that if pebbles or pepper had been swallowed recently before death, a considerable quantity of thinner ropy mucus, and an increased quantity of bile had been poured out; and Leuret and Lassaigne further observed, that, when the villous coat of the duodenum was exposed and cleaned, and then touched with diluted vinegar, the membrane immediately exhaled a clear fluid, and the *choledochus* duct discharged much bile and pancreatic juice. It must, however, be evidently almost impossible to acquire a correct idea of the composition and properties of the intestinal secretion, as it cannot be procured free from bile and pancreatic juice. Tiedemann and Gmelin found that, in animals which had swallowed pebbles while fasting, there was in the intestinal contents more albumen than the pancreatic juice could account for; and Leuret and Lassaigne also found that the matter adhering to the intestines is in similar circumstances faintly acid. Both believe that it possesses the power of dissolving the food, and Leuret and Lassaigne even give experiments from which they wish it to be inferred, that the intestinal secretions, when mixed with the bile, form a solvent for the food quite as active as the gastric juice, if not even more energetic. But their experiments are unsatisfactory, because in the way in which the intestinal contents were collected they could not fail to be mingled with gastric juice.

But whatever may be the agent or agents in the process, it is well known that in their course through the intestines the portions of food which escape chymification in the stomach are gradually altered in nature. The more nutritive articles, such as coagulated albumen, fibrin, casein, gelatin, disappear entirely, according to Tiedemann and Gmelin, before they reach the lower end of the ileum, and when any fecula passes the pylorus unaltered it is converted as in the stomach into sugar and amidine. There is, therefore, a close analogy between the process of chymification as carried on in the stomach, and the changes which are effected in the small intestines.

On account of the great number and the complexity of the fluids which are added to the chyme after it reaches the intestines, it is very difficult to determine what precise changes are effected on the food after chymification. The chief facts determined by our author are, that the acidity of the chyme decreases as it passes downwards, and at length disappears long before it reaches the *cæcum*; that albumen exists abundantly in the duodenum, and decreases rapidly downwards; and that the casein and other highly azotized principles contained in the pancreatic juice also gradually disappear. The disappearance of the acidity of the chyme is partly explained by the admixture of the alkali contained in the bile; the albumen is lost, because

it forms an important element of the chyle; and Tiedemann and Gmelin suppose that the other more highly azotized principles go to convert into albumen the unazotized principles in vegetable food,—an idea which derives some support from the fact that herbivorous animals have a larger pancreas than carnivorous animals, and that a corresponding difference exists between the wild and domestic cat, the former of which lives entirely on animal food, and has the least pancreas.

The changes wrought on the food in the intestines take place chiefly in the duodenum and upper part of the jejunum. This is shown by the visible properties of the contents of the intestines, and by chemical analysis, and it is farther proved by the gases evolved in the small intestines being produced, according to Leuret and Lassaigne, chiefly, if not solely, in the upper part of their course. These gases, according to the same experimentalists, consist of carbonic acid, azote, and carburetted hydrogen, and are the result, not as some think of putrefaction, but of the chemical changes caused by the digestive process.

Tiedemann and Gmelin maintain that it is an error to suppose, as some physiologists, and very recently Magendie, have done, that chyle is formed, and may be seen in the intestines. White flakes may be remarked floating in their contents, and adhering to their inner surface, and these have been believed to be the chyle on the point of being absorbed. They are certainly not formed, as has been imagined, by the action of the bile on the chyme, for no such effect is caused by the bile on the chyme out of the body; and on the whole, it appears that they are really nothing else than flakes of thickened mucus. Leuret and Lassaigne, however, affirm, that all the essential principles of the chyle exist ready formed in the chyme. The serous part of the chyle is well known to be present. The fibrinous particles, they say, may also be detected. It is in vain, indeed, to attempt to discover them by chemical analysis; this cannot be done even when chyle is purposely mixed with chyme,—the proportion of other principles mixed with them being too great. But if the chyme be attentively examined with the microscope, globules will be found even in the stomach, but much more numerous in the duodenum, which resemble exactly the fibrinous globules of the chyle. That they are in fact nothing else, appears, they think, from their being seen before the microscope to run together and form fine fibrils, and from the absence of such globules in the gastric juice, bile, pancreatic fluid, and intestinal mucus, so that nothing but the food can yield them. If these remarks shall be confirmed, the Parisian phy-

siologists have certainly added an important fact to our knowledge of the process of chylification.

Having thus seen what light the researches of our authors have thrown on the more intimate changes which the food undergoes in its course downwards till it reaches the end of the small intestines, it now remains for us to point out their leading observations on the ulterior processes which are carried on in the cœcum and lower part of the great intestines.

The *Cœcum* is very generally believed to be as it were a subsidiary stomach, in which the portions of the food which escape assimilation in their rapid passage through the small intestines are detained, and undergo digestion. Professors Tiedemann and Gmelin have added several new facts to the arguments formerly adduced in support of this idea. Thus they have found that the intestinal secretions, when excited by the presence of pebbles, though not acid in the lower part of the small intestines, become distinctly so in the cœcum; that its contents after a meal are also acid, and the more strongly so, the more indigestible the food is; and that albumen, which gradually disappears in the contents of the small intestines, reappears often abundantly in those of the cœcum. The other principal ingredients in the contents of that part of the canal are a matter resembling osmazome, a substance which is characterized by being reddened by various acids and metallic salts, and fatty matter, resin of bile, colouring matter of the bile, sometimes too cholesterine.

The changes which the contents of the cœcum undergo in the *Colon* and *Rectum* have not been particularly examined in either of the works before us. They evidently consist, however, chiefly in the indigestible part of the food, biliary resin, colouring matter, fatty matter, and intestinal mucus, forming a mass which becomes more and more consistent as it approaches the aperture by which it is discharged.

We have in the last place to relate the observations which have been made on the *Chyle*.

Both MM. Leuret and Lassaigne, and Professors Gmelin and Tiedemann, agree in all the material parts of the analysis of the chyle, as executed by the late Dr Marcet. What follows is an analysis of the researches of the German professors. Chyle is now well known to consist of a serous and a fibrinous part, the latter of which separates like that of the blood by spontaneous coagulation. The firmness of the coagulum seems to depend chiefly on the quantity of fibrin. Chyle hardly coagulates at all before it has passed through the mesenteric glands. After passing through them, the fibrin begins to appear, and it is much more abundant after the addition of the lymph from the

spleen, which contains a very large quantity of fibrin. The quantity is considerably lessened in the chyle of digestion; and it is increased in the chyle formed after the ligature of the *ductus choledochus*. It abounds in the lymph from the lower extremities. In like manner the chyle before passing the mesenteric glands contains no red particles; but it does immediately afterwards, and more particularly after it is mixed with the lymph from the spleen, which abounds with them as with fibrin. These particles are also, like the fibrin, very much diminished in the chyle of digestion, and proportionally to the nutritiveness and digestibility of the food. They are increased by tying the choledochus duct. They abound in the lymph of the lower extremities. The chyle frequently contains fatty matter,—very little or none, however, if the animal is fasting, or has fed on food which does not contain fat,—and most when the food is very fatty, when, for example, butter is mixed with it. The fatty matter is not dissolved, but exists merely in a state of minute division and suspension, giving to the chyle its peculiar white colour; for the colour is removed, and the chyle rendered limpid by ether, which carries away the fatty particles. There is no fatty matter in the lymph of the lower extremities; it is much less abundant in the thoracic duct than in the chyle before it passes through the mesenteric glands, and it hardly exists in the chyle at all when the *ductus choledochus* is tied. The serum of the chyle is very generally alkaline; in two instances only, was it found neutral, namely in a dog fed on fibrin, and in a sheep fed on oats. Its solid contents differ in the chyle of fasting animals and in that of digestion. In the horse while fasting the solid part of the serum consists on an average of 76.2 per cent of albumen, 6.7 animal matter soluble in water, and 16 animal matter soluble in alcohol; but after digestion of 61 albumen, 3 animal matter soluble in water, 34 animal matter soluble in alcohol, of which twenty parts were fat. Our authors were not able to decide whether the total amount of solid matter in the serum is increased or diminished during digestion.

The inference drawn by Tiedemann and Gmelin from the foregoing facts is, that the fibrin, colouring particles, and albumen of the chyle, are supplied either not at all by the intestinal lacteals, or at least in much less quantity than by the lymph, which is formed by the blood, and that the food supplies chiefly fatty matter, and other principles soluble in alcohol, particularly osmazome. Before they are entitled, however, to form these conclusions unreservedly, it is necessary to establish a preliminary condition, which they have entirely neglected, namely, that the flow of the lymph increases along with the flow of the intestinal chyle during digestion; for if it does

not, then the proportional deficiency of albumen and fibrin in the chyle of digestion, when compared with the lymph, is no proof whatever that the former does not supply even more of these principles than the latter to the blood. The inferior proportion may be more than compensated by the great increase in the quantity of fluid.

MM. Leuret and Lassaigne, we must add, have remarked that chyle is taken up from the stomach as well as the intestines. Physiologists, they say, imagined that chylification commenced in the duodenum, because the addition of the bile was considered necessary. But if an animal be examined soon after digestion has begun, true chyle will be found in the lymphatic vessels of the stomach.

On reviewing the outline now given of the result of the competition for the academy's prize, the physiologist cannot fail to perceive that his science has reaped from it a rich harvest of facts and observation. No great discovery indeed has been made. Yet at the same time many interesting novelties have been brought forward, many gaps in former researches filled up, many doubts and obscurities cleared away; and on the whole, it cannot be denied that the physiology of digestion has in consequence received much illumination, and that a theory may now be established where there formerly existed nothing else than a hypothesis. The nature of the saliva has been settled; the composition and solvent power of the gastric juice, so much disputed, are now ascertained; the composition of the bile has been more minutely examined, some of its purposes fixed, and certain conjectures as to other uses disproved; the function of the cœcum has been established on surer grounds than formerly; the more intimate chemical changes which the food undergoes in the course of chymification have been to a certain extent unfolded; and much light has been thrown on the circumstances which regulate the varying nature of the chyle.* The chief points which are still left in doubt are the following. The nature and purposes of the pancreatic juice are still unsettled: the changes which the chyle undergoes in its course till it enters the blood, and the agents which effect those changes still supply an ample field of research; and in regard to the chemical changes of the food as it passes through the alimentary canal, there is still much left to be determined when the progress of organic chemistry shall have supplied the physiologist with new modes of investigation.

* Both the German and the Parisian experimentalists, but particularly the former, have studied the digestion of other tribes of animals besides that of man and the *mammalia*. It is impossible, however, for us to command space enough to notice their observations.

ART. II.—*The Dublin Hospital Reports, and Communications in Medicine and Surgery.* Volume IV. Dublin, London, Edinburgh, 1827. (*Continued from Vol. xxvii. p. 334.*)

IN resuming the examination of this volume it will be most convenient to notice the several papers in the following order, as they treat of subjects in morbid anatomy, subjects in medicine and medical pathology, and subjects in surgery and surgical pathology.

Under the first head three papers are presented to the reader.

In the *account of an unusual variety of the Femoral Artery*, Mr Houston describes a singular subdivision or splitting of the superficial femoral artery into two parallel branches, which again, at perforating the tendon of the *triceps*, reunited to form the popliteal in the ordinary manner. These vessels, though each enveloped in a separate sheath, preserved the same relation to the contiguous parts as the femoral which they represented does. The external branch sent off three vessels to the muscles and adjoining parts; the internal sent off a single small one, which was lost in the adductor muscles.

A similar case occurring in the person of a negro, who underwent the operation for popliteal aneurism in the Middlesex Hospital, is published by Mr Charles Bell in Anderson's Quarterly Journal for October 1826.

In the *description of a human stomach of singular form and structure* by Mr Hart, that organ was situate more in a perpendicular than its usual transverse direction; while the first portion of the duodenum was unusually long, and, instead of ascending as usual, proceeded transversely to the right in its course to the neck of the gall bladder. Upon further examination, the right extremity, instead of contracting gradually to the pyloric orifice, was expanded into a *cul-de-sac*, about half as large as that of the left. This expansion naturally enlarged and lengthened the great arch, which thus was made to unite with the small arch at the upper surface of the organ; and from the angle of union thus formed the duodenum was abruptly given off.

In the inside the pyloric orifice was not as usual circular, but consisted of a valvular slit, lying transversely to the line of the small arch, very similar to the ileo-cæcal valve in appearance. Mr Hart regards this variety in the arrangement and structure of the human stomach as very similar to that of carnivorous animals.

In the third paper, Dr Jacobs describes a *lusus*, or monstrosi-

ty, in the sacral bones of a young man. From the situation which it occupied this was supposed to be an example of the tail, for the existence of which, as a natural appendage of the human body, so many predecessors and followers of Lord Monboddo have at different times contended. When divided by a longitudinal section, it was found to consist of bony pieces, articulated with distinct synovial capsules. The bones, however, bore no resemblance to vertebræ, and might have been as justly regarded, in Dr Jacob's opinion, as tarsal and metatarsal bones with phalanges. As they formed one series of bones, however, instead of five, this opinion becomes less probable. It might, on the other hand, be inferred, without much violence to the ordinary laws of organization, that these bones were the rudiment of an imperfect sacrum. This explanation would be equally fatal as the other to the idea of the tail; since, according to the principles delivered by Meckel, the monstrosity here remarked may be viewed as the result of a very slight aberration in the early growth of the fœtus.

Of the medical papers, the first which claims attention is a *Medical Report on the Feigned Diseases of Soldiers, &c.* by Dr John Cheyne, physician-general to his Majesty's army in Ireland.

It is no inconsiderable proof of the active and original spirit of the profession in this country, that the department of medical science which treats of feigned diseases, and the means of detecting them, has been, if not created, at least brought to a very high degree of perfection within the last ten years, entirely and exclusively by British physicians. The practice of feigning is indeed not new or of recent origin; and Ambrose Paré has related a few extraordinary instances of deception, which were current and notorious in his days. These, however, are to be viewed rather in the light of marvellous tales of the disgusting knavery of beggars, and the malicious ingenuity of the most depraved of mankind, than as rational and systematic accounts, from which the regimental surgeon is to derive the information which may enable to guard against deceit, and detect imposture. The loathsome tricks practised in the days of Paré are too coarse to escape very ordinary observation; and without a more refined system of deception the impostor of the present day could neither effect his purpose, nor cause much trouble to his medical attendant.

It appears to have been in the course of the first or revolutionary war, previous to the peace of Amiens, when the medical department of the army was imperfectly organized, that the practice of feigning sick first began to prevail to an alarming

extent; and it was during the subsequent period of warfare that the attention of the army-surgeons was forcibly directed to the exposure and suppression of the practice. In the useful volume published in this city by Dr Hennen, the surgeon finds the first connected account of several of those diseases, which are too often simulated by the soldier for the purpose of evading military duty, or obtaining a discharge from the service. Of the essay of Mr Marshall, published a twelvemonth ago in this Journal, it is sufficient to say that it is the work of a diligent army-surgeon, who has spent the greater part of his professional life among soldiers,—who has great experience in the inspection of recruits,*—and whose natural acuteness and habits of attentive observation have been long directed, with unremitting energy and perseverance, to the means of distinguishing genuine from fictitious complaints. The observations of Mr Copland Hutchison, which were noticed in a former number, communicate much curious and useful information on the tricks practised by seamen, and the complaints with which they frequently pretend to be afflicted, and reflect much credit on the diligence and acuteness of that ingenious observer.

In the present essay, which is in the form of a letter addressed to Dr Renny, the Director-General of Military Hospitals in Ireland, though Dr Cheyne treats the same subject, yet he has made some valuable additions to the amount of knowledge already before the profession. Conversant more or less intimately with sick soldiers for a period of twenty-two years, he has had ample opportunities of acquiring a knowledge of their characters and habits, and of observing the privations which they undergo, and the sacrifices to which they not unfrequently submit, in feigning disease, or inducing it by artificial means, in pursuit of a favourite object. According to the experience of Dr Cheyne, the practice of feigning sickness is most frequent among the Irish, who are very expert in the art, next frequent among the lowland Scotch, whose obstinacy supplies the want of address, and least of all among the Highland regiments and the English soldiers. In some regiments, particularly of the cavalry service, and several of the infantry battalions, where high discipline and a sort of regimental reputation have concurred to give the men an unusual elevation of character, the practice of counterfeiting illness is utterly unknown. In other regiments again, which have been hastily and therefore indiscriminately recruited, or which have been reformed under circumstances unfavourable to perfect discipline, it is so thoroughly understood, that it

* We know on good authority that Mr Marshall has examined 15,000 recruits.

appears to be reduced to system, and transmitted through successive crops of men for the benefit of those who are naturally suited or inclined to put it to trial. It seems to be chiefly among the latter that the system passes, in the slang of the military hospital, originally derived perhaps from an Irish stock, under the name of *malingering*, and that the soldier who is understood to counterfeit is denominated a *malingerer*. These terms are so generally known, and so useful in application, that Dr Cheyne scruples not to stamp them with currency in his observations on the practice which they are employed to designate.

The military nosology is not extensive; but the soldier exercises considerable ingenuity in the choice of the disease which he undertakes to counterfeit, and he may for some time overcome, by natural quickness and acquired address, the actual difficulties of the task of feigning sick. Medical diseases are in general convenient in being detected with difficulty; but to counterfeit them for any length of time successfully requires much knowledge, which can only be procured by long and attentive observation of the unfeigned complaints of genuine patients. In this way chiefly, soldiers who feign are either those who have been long in the service, or those who have been often in hospital; and for the same reason they find it most convenient to chuse some of the most ordinary and frequent diseases incident to the military life. The diseases which are counterfeited in this manner are considered by Dr Cheyne in the following order.

A. Diseases of the brain and nervous system. 1. Palsy, partial, of a member, loss of speech, inability to command the sphincters; 2. impaired sensation, *e. g.* loss of hearing, amaurosis, nyctalopia; 3. vertigo and headach; 4. epilepsy; 5. mania.

B. Diseases of the thoracic organs. 6. Hæmoptoe; 7. phthisis; 8. diseased heart; 9. syncope.

C. Diseases of the abdominal organs. 10. vomiting; 11. tympany; 12. dysentery; 13. chronic hepatitis.

D. General diseases. 14. fevers; 15. rheumatism; 16. dropsy.

It is unnecessary to follow the steps of Dr Cheyne in his description of the mode in which each of these complaints is simulated, of the varied means employed to deceive the surgeon, and of the mode by which they may be detected. It is enough to advert to a few of the most important, as specimens of the manner in which the subject is treated by the author, and to direct the attention to the peculiar views which he takes.

Palsy.—In simulating varieties of this affection the perseverance and ingenuity of the malingerer are often extreme. An arm, a leg, or both legs, may be pretended to have become paralytic, and notwithstanding all the torture of cupping, blister-

ing, cautery, and electricity, seem to remain incurably motionless. If the want of affection of the brain, or a corresponding affection of the opposite limb, give rise to suspicion, smart shocks of electricity are the most likely means of extorting confession of the true state of the limb. In some instances the ingenuity of the attendant may, by taking the individual by surprise, throw him off his guard and detect the imposture. In other cases, however, the perseverance of the malingerer triumphs over every method; and when he attains his purpose, he fails not to display his exultation in the most shameless and public manner.

“About twenty years ago, a trooper belonging to the 12th, who pretended that he had lost the use of his right arm, after resisting for a long time severe hospital discipline, as well as every effort of the adjutant to make him return to his duty, succeeded in procuring his discharge. When he was leaving the regiment, and was fairly seated on the top of the coach, he waved the paralytic arm in triumph, and cheered at the success of his plans.”

“A militia soldier, who pretended that he had lost the use of his inferior extremities, was reported unfit for service by the late Dr Harvey, and discharged. When he had obtained possession of his discharge, he caused himself on a field day to be taken in a cart to the Phoenix Park, and in front of the regiment, which was drawn up in line, he had the cart drive under a tree, upon which he hung up his crutches, leaped out of the cart, sprung three times from the ground, turned his back to the regiment, and having slapped his breech, he scampered off at full speed.”

Inability to retain the feces, and incontinence of urine, are often pretended by soldiers who wish to perplex their officers or annoy their comrades. In a case of the former, which occurred in the general hospital at Lisburn, the actual cautery was a successful remedy; and when the latter affection was extensively feigned in the Dublin militia in Antrim in 1802 and 1803, it completely disappeared under the discipline of marching the pretended patients in a body every morning and evening, at six o'clock, to Lough Neagh, and bathing them in the cold lake.

Epilepsy is often pretended, and is not easily detected. Dr Cheyne recommends attentive observation of the previous, the concomitant, and the succeeding symptoms of the fit, and its collateral or vicarious circumstances, whether it precedes or alternates with catalepsy, hysteria, a sub-epileptic attack, or a distinct maniacal accession, or is connected with palsy or fatuity. The most effectual method of exposing the pretended epileptic, is by dropping into the eye, through a quill, a few minims of any of the ordinary spirituous tinctures, the smarting of which is so insupportable as to disconcert the pretender, and make him either desist from his contortions, or practise them so irregularly as to show at once their real nature.

Consumption, with or without its common symptom, hæmoptoe, is often feigned by the soldier with considerable dexterity. A man under treatment for catarrh, or recovering from fever with pulmonary irritation, all at once becomes stationary in his convalescence; he coughs at the period of visit; complains of pain in the breast or side; says his food stuffs him, and wishes to be placed on spoon diet, and to lose blood. In such cases attentive observation of symptoms, and the use of the stethoscope, will probably show that no pathological cause for such complaints exists; and by properly speaking to the patient, the surgeon will induce him to acknowledge that the complaints are either assumed or exaggerated.

Consumption, however, is so often an unfeigned disease of the soldier, that it causes more than one-third of the whole mortality of the army, and, including those who die after being invalided, it may be computed to produce one-half of the mortality of the army in Ireland. In a large proportion of those who become victims of this disease the chest is narrow and badly formed; and the habits and duties of the service have a natural tendency to bring into operation its exciting causes. It becomes an indispensable duty, therefore, in the estimation of Dr Cheyne, to reject every recruit whose chest is not well-formed. The accessory utility of the stethoscope in this inquiry is obvious.

Vomiting is known to be intentionally produced by swallowing air, which is afterwards returned with the food, or by swallowing tobacco juice. Under this head Dr Cheyne relates two different cases, which forcibly demonstrate the necessity of the utmost caution and deliberation in distinguishing between fictitious and unfeigned disease.

In the first, a young man of reputed good character, and open and intelligent countenance, but pale and delicate, was admitted into the King's Infirmary for alleged stomach complaint, the chief symptom of which was uneasiness after meals, terminating in vomiting. As the complaint had continued for two or three years, during which he had become burdensome to the corps, by the frequent recurrence of his fits of indisposition, he was, after a course of medicine, reported as unfit for service, and was consequently discharged. No sooner was the report of Dr Cheyne, as physician-general, forwarded, than the man's appearance improved considerably; and about a fortnight after he left the hospital the doctor met him in the street in vigorous health. As the result of this case had convinced the physician-general that this man was an accomplished impostor, and had availed himself of some of the usual means of feigning sickness, he was prepared to expect similar tricks from similar patients. But his circumspection was destined to be once more

at fault. About the same time a poor sickly lad, recently enlisted, was sent to the infirmary with a similar complaint. Though not emaciated, his skin was dingy, his pulse slow, and he complained of uneasiness in the stomach, and constant vomiting. Though the use of remedies afforded him relief, he returned to hospital a few days after with the same complaint; and as his delicate, spiritless, and miserable appearance gave no promise of his becoming a useful soldier, Dr Cheyne reported him for discharge, which took place accordingly. The boy, however, was destitute, and he threw himself on the establishment, where he died in a few days. After death the mucous membrane of the stomach was found every where pulpy and varicose. "In neither of these cases," says Dr Cheyne with great candour, "had I formed a correct judgment; the knave accomplished his purpose, while the poor suffering boy was treated with unjust suspicion. I am happy, however, to say, that, though for a considerable time he was thought to be a malingerer, he was not treated with any severity."

Tympanitic swelling of the belly, though rather an extraordinary affection, as it is often dependent on organic disease, is nevertheless often simulated by the soldier for the purpose of obtaining a discharge. The Indian method mentioned by Mr Marshall, of swallowing large quantities of congee-toddy with soap, is represented by Dr Cheyne as too well known to succeed. Many soldiers, however, acquire the power of swallowing air and distending the belly. In the 84th, it was said to be produced by swallowing immense quantities of chalk and vinegar; and in the 35th, Mr Bampffield saw three cases in which it was attempted by forcing down the diaphragm by a deep inspiration, and keeping up the distension by very short expirations. It is in general easily detected by attentive and repeated inspection. But when this fails, the expedient of Dr Harvey with the men of the second battalion of the 84th in 1811 affords the best prospect of success. About forty of these men had tympanitic swelling of the belly, which they reported to be more or less painful, with excessive thirst, and constipated bowels; while the tongue was, with few exceptions, clean, the pulse regular, and the urine natural. This was at first taken for a genuine disease, the effects of the Walcheren fever; and sixteen men had actually procured their discharge. As the numbers increased, however, and all with uniformly the same symptoms, Dr Harvey was led to think the complaint entirely simulated. Under this impression he prescribed a solution of Glauber's salts in weak tobacco water, a cupful of which mixture was given in the morning, and repeated every fourth hour till it operated. The effect of this palatable remedy was wonderful. All

the patients in hospital recovered speedily; and the disease, which was becoming epidemic, soon disappeared.

In speaking of feigned *chronic hepatitis*, Dr Cheyne strongly and unconditionally reprobates the practice of giving mercury, as this not only enables the malingerer to mature his plans of imposture, but inflicts a real injury on his constitution. He also takes occasion most justly to condemn the fashionable empiricism of ascribing all intestinal complaints to diseased liver, and the consequent practice of administering blue pill, or dosing the system with mercury to cure a disease, the existence of which is problematical, or altogether imaginary. All that can be said in favour of this systematic and universal mode of treating complaints of the digestive organs is, that it saves the medical attendant a great deal of trouble, either in examining his patient or in exercising his faculties in tracing his disease, and managing the peculiar features of each case by appropriate remedies.

The present essay is distinguished for a remarkable peculiarity in the observation of the feigned diseases of the soldier, which cannot be too generally known, or too strongly impressed on the mind of the regimental surgeon. The great prevalence of this practice at certain periods of the last war, especially previous to the peace of Amiens, when the organization and discipline of the army were inferior to what they became in 1810 and 1811, and its notoriety in certain regiments led some army physicians and regimental surgeons to overrate its extent, and by consequence, to suppose in individual cases feigning to be more frequent than it is in truth. Dr Cheyne displays a laudable anxiety to guard against this error, which is not less pernicious than the extreme facility of medical officers about thirty years ago. He takes frequent opportunities to impress on the mind of the regimental surgeon the possibility of a disease which is suspected to be feigned being quite genuine, and of its unfortunate subject being either allowed to become its victim, or submitted during its progress to injurious neglect or unnecessary and wanton torture. The reader is already in possession of one case in which Dr Cheyne candidly avows that the suspicion of feigning fell unjustly on a poor young man who laboured under unfeigned disease of the stomach. He also takes occasion to mention a case in which listlessness and inattention to duties had caused an active hussar to be considered as a *skulker*, and eventually discharged; but in whom these phenomena were the incipient symptoms of disease of the brain, which at length proved fatal. In another instance, a private of the 10th was employed in 1823 by an officer of that corps as bat-man, but who, for a slovenly way of going about his work, which was

ascribed to laziness, was sent back to duty. Shortly after, when he was admitted to the hospital, he was found to have diseased spleen, which was soon followed by general dropsy. In a third case in 1805, a soldier of the 9th, who complained of uneasiness in the loins, was suspected to be feigning, and was therefore sent to punishment drill, at which he was kept till a tumour, the usual symptom of lumbar abscess, appeared in his back, and soon terminated his existence.

Cases of this description are sufficiently flagrant if they occur once or twice in the course of a series of years. But what reflections must arise when we are told that "it would be tedious to multiply cases of this kind;" and that "he had known of several examples of affections of the brain, of the chest, of the belly, and diseases of the hip-joint, supposed at first to be feigned, eventually proving genuine, and leading to death or incurable disease?" It is to be hoped, for the sake of the army surgeons and their medical inspectors, that this evil does not prevail to the alarming extent here represented. It is certainly an enormous abuse to give the practice of feigning any encouragement whatever, by relying on all the miserable grounds for which unprincipled men have such a prompt and ready talent. But it is, if possible, a greater abuse to carry incredulity so far as to neglect the complaints of genuine disease, and allow the innocent to suffer the punishment of the deceit of the criminal. It is, however, important for the humane army-surgeon to know that this is not a necessary evil of the practice of feigning. Dr Cheyne has been at much pains to show that the duty of the regimental surgeon may be divested of all harshness and unnecessary responsibility by proceeding carefully and circumspectly in every case of suspected feigning; and he has digested a series of rules for the regulation of his conduct, which ought to be generally known and reduced to practice.

"1st, The medical officer must not allow even flagrant imposition to deprive him of the command of his temper; he must listen to the most contradictory statement, not merely with patience, but without evincing the slightest distrust. In short, his manner must be the same to a soldier labouring under strong suspicion of fraud, as it would be to the best man in the regiment, and he will in general find that complete ignorance of his sentiments will, more than any thing, disconcert the malingerer.

"2dly, If the case is evidently feigned he ought to take the malingerer aside, mildly expostulate with him on his folly, or, if necessary, threaten to report him to the commanding officer if he should persist in his misconduct, or again attempt to feign sickness. By such means many a good soldier has been reclaimed, who, had he been exposed to shame, would have become a callous profligate.

"3dly, If he should fail by means of persuasion, and if the fraud

be palpable, he ought to take the malingerer into hospital, and, without prescribing for his pretended complaints, lay the case before the commanding officer.

“ 4thly, But if the grounds of his suspicion cannot be convincingly stated, he must cautiously conceal his sentiments, until by patient investigation his doubts are removed, and a satisfactory report of the case can be prepared.

“ 5thly, In this stage of the inquiry he must employ no means but such as would be applicable to the case were it genuine. He must not, on his own authority, employ any coercive or penal measures, not even irritating applications, nauseating medicines, nor spare diet, unless such would be proper were the disease real.

“ 6thly, When, after the calmest inquiry, he is convinced that the complaint is unfounded, or the disease fabricated, and shall have reported accordingly to the commanding officer, the case is no longer in his hands ; he ought not to prescribe for the malingerer, but ought to pass him in going through the wards. Neglect will often bring him to resume his duty. The commanding officer, if he be a judicious man, and repose confidence in the surgeon, will take the advice of the latter relative to the treatment of the malingerer, and will authorize any measures which medical experience may suggest. Restraint or punishment in hospital, under the sanction of the commanding officer, may, in some cases, be useful.

“ 7thly, If the malingerer should still persevere in a course of imposition, it is customary to bring him before a court-martial. It would appear to me that a medical board, consisting of at least three medical officers of mature experience, would be found the better tribunal in the first instance. If the medical board should report favourably of the accused, he will be discharged from the service, and the corps relieved of a clog ; if unfavourably, he must then abide the sentence of a court-martial.

“ To suppress malingering is an important part of the discipline of a corps, in which the surgeon must assist to the utmost of his ability. His conduct, however, must be guided by just principles. He must avoid all harsh, arbitrary, and unauthorized proceedings. After having stated his opinion, and the grounds of it, and given his advice when required to do so, the case is no longer in his hands ; all further measures must originate with the commanding officer, with whom the responsibility of these ought solely to rest.”

In the second part of the volume, consisting of miscellaneous communications, the second medical paper is by Dr Colles, and forms an appendix to his former communication in Vol. iii. p. 207, on the fatal consequences of slight wounds received in dissection. The paper contains the melancholy case of Mr Sherkelton, who perished by this disease in May 1824, with some observations by the author. The case is, on the whole, so similar to the others which are already published, that though it confirms all that has been observed in this extraordinary and uncontrollable malady, it is unnecessary to dwell on it in this

place. The most singular phenomenon in this case was yellowness of the skin, appearing on the face the day before death, and spreading thence very rapidly over the whole person.

Dr Colles does not agree with Dr Duncan Junior in placing the seat of this affection in the cellular tissue. He thinks mercury given to produce salivation the most probable means of affording benefit, and arresting the morbid action. But though this is a point which cannot be determined without extensive trials, in the course of which, doubtless, many cases would die under any method of treatment whatever, there is no reason to suppose that mercurialism would be more efficacious than other means.

Dr Cheyne describes three cases of what he terms *Fatal Erethism of the stomach*; but this must be regarded as an instance of the figure of speech called *catachresis*, since *one* of them did not terminate fatally. The symptoms were incessant and uncontrollable vomiting of a ropy tasteless liquor, the colour of verdigris, much thirst, great weakness and anxiety, without obvious pain of any part of the belly, and without quickness or sharpness of pulse, or the other usual symptoms of fever. In the only fatal case in which dissection was performed, the mucous membrane of the œsophagus was deep-red and highly vascular; the mucous membrane of the stomach, particularly at the large extremity, of a dark mahogany colour, arising from vascular distension; the veins were unusually turgid; and there was extravasation into the submucous tissue generally; and the same appearances, though less in degree, were remarked in the intestines.

As these four cases occurred in individuals of the same family; as a brother and a sister, and even the father, had died with similar symptoms; Dr Cheyne infers that the disease, to which he applies the name of *erethism*, depends on hereditary or family peculiarity, probably of a strumous character.

Dr Davis gives the particulars of the case of a girl of six years, labouring under symptoms of acute water of the brain, complicated with a petechial and miliary eruption on the person, and in whom all the veins were the seat of a distinct pulsation, synchronous with that of the arteries. The dissection throws no light on this phenomenon, which Dr Davis seems inclined to ascribe to the pulsation of the heart being continued through the arteries into the veins.

The next medical paper consists of *Additional cases* from the medical wards of Meath Hospital by Drs Graves and Stokes, and forms a continuation of that given in the former part of the volume by the same gentlemen. These cases are one of gangrene of the lung; one of pulmonary apoplexy; each with the

necroscopic appearances; two of disease of the skin, with observations; and one of fatal sloughing of the mouth and palate from a small dose of calomel, with some remarks on the propagation of inflammation by contiguity. Into the detail of these cases it is impossible here to enter. But the reader will admit that this is superfluous, when he is told, that, to do them justice, they should be studied in the words of the authors. The two first are exceedingly good instances of the diseases which they are intended to exemplify.

In the next medical paper, Dr John Crampton describes the particulars of a case in which, after an attack of intestinal inflammation, the bowels remained constipated; first, for eight or ten days at a time, afterwards for the space of eight months, yet without terminating the existence of the individual. The case is imperfect, insomuch as the patient still lives, and therefore the state of the bowels cannot be ascertained. The conjecture advanced by Dr Crampton is, however, highly probable, and derives confirmation, not only from the state of the colon in the case of Anne Free, recorded by the author, but more particularly from two cases recorded by Dr Baillie and Dr Willan, in both of which considerable stricture was found to have taken place. If Dr Crampton shall subsequently publish the dissection of the subject of his case, the pathologist will derive much instruction from comparing it with the case of Anne Free, and those given by Dr Willan and Dr Baillie.

The *observations on an affection of the mouth in children* by Dr Thomas Cuming furnish some useful information on the leading varieties of the disease which is known under the name of *Cancerum oris* or Water-Canker. This affection, which was first well described by Van Swieten, and has since his time exercised the observing powers of Dr Robert Hamilton of Lynn Regis, Dr Marshall Hall, and others, consists in malignant inflammatory action, terminating more or less speedily in phagedenic ulceration, with its usual attendants of sloughing, hemorrhage, caries of the alveolar processes, and elimination of the teeth. Dr Cuming refers the different forms of the disease to three varieties.

The first is that which occurs in the gums of infants at the breast, and under twenty months. It consists in acute inflammation of the gums, proceeding rapidly to ulceration, but rarely with sloughing.

The second variety, which occurs in children between twenty months and seven years of age, is distinguished by its inflammatory and ulcerative character, being accompanied with more or less sloughing. From its close resemblance in many instances to the effects of mercury when operating as a poisonous agent, it might be imagined to depend on the fre-

quency of exhibiting calomel in treating the complaints of children. This opinion, however, Dr Cuming thinks, is contradicted by the fact, that in many cases no connexion between the appearance of this disease and the administration of the mineral could be traced.

In the third variety, though the disease appears to commence at once with ulceration of the lining membrane of the cheek and lips, this is speedily followed by a hard, red, shining, swelled border, which, if the morbid action be not arrested, speedily passes into gangrene. In the rapidity of progress, the predominance of the gangrenous destruction, and the severity of the constitutional disorder, Dr Cuming conceives this malady to bear a close resemblance to the gangrenous inflammation of the pudendum in infants, first described by Dr Percival, and afterwards by Mr Kinder Wood.

This affection is well known to be extremely rapid, and in a large proportion of cases to prove fatal whatever remedial means be attempted. Dr Cuming is inclined to trust to evacuation of the bowels by calomel and jalap, followed by the tonic influence of sulphate of quinine to the amount of six grains, with three of Dover's powder daily. He also uses carbonate of ammonia in doses of four grains every third hour, at the suggestion of Mr Wallace, and so far as can be judged with good effect.

Mr Spcer relates a case of rupture of the cœcum, occasioned by a violent contusion of the belly during a fall, and which terminated fatally forty-eight hours after the accident. The symptoms were those of peritonitis, arising after perforation of the intestines. The aperture of the cœcum was about two inches in circumference, with uneven ragged edges, which were surrounded with marks of extensive inflammation. The peritoneal coat of all the small intestines was also thickly covered with coagulable lymph.

The next article in the volume, consisting of cases of diseases of the heart, with pathological observations by Mr Robert Adams, is long, elaborate, and stored with useful information. The author considers, first, some morbid affections of the coverings of the heart; secondly, the changes incident to its muscular structure; lastly, those organic changes of the auricular and ventricular openings which derange the function of circulation,—all illustrated by cases.

Under the first head Mr Adams treats of ossification of the pericardium, and relates a good example of this organic change;—pericarditis illustrated by two cases;—rheumatic inflammation illustrated by one example;—rheumatism of the heart, and erysipelas of the heart. Experience leads Mr Adams to regard rheumatism as capable of producing two morbid states of the heart, quite distinct from each other in the nature of the af-

fectured tissue, and in severity and danger. Rheumatism, he thinks, may affect the fleshy substance of the heart, and produce disease severe and dangerous indeed, but much milder, slower, and more manageable, than when its serous membrane is affected. This form of disease, he thinks, takes place only when rheumatism is originally seated in the synovial membranes of the tendinous sheaths and the joints, and passes thence by sudden metastasis to the pericardium, in which it kindles at once genuine inflammation, with all its symptoms and effects, and unless energetically and promptly opposed by remedies, quickly proceeds to the fatal termination. It is an interesting proof of coincidence, if not analogy, that the case given by Mr Adams occurred in a girl of six, and the best example of the disease which fell under our observation took place in the person of a girl of thirteen. This perhaps may afford one explanation of a fact remarked by Fothergill, that rheumatism in young persons often proves suddenly and unexpectedly fatal.

In treating the subjects of the second division, the most important point advanced by Mr Adams is the confirmation given to a doctrine advanced by various authors of late years, on the connexion between disease of the heart and apoplectic or lethargic death. Mr Adams details two cases of enlargement of the heart, and one of adipose degeneration, in which life was terminated by apoplexy; and in the two last of which the brain was infiltrated with watery effusion. In the first, and in one in which the patient had been long delirious, it seems to be an unfortunate omission that the brain was not examined.

In two cases of rupture of the heart the laceration was found in the anterior part of the left ventricle. This affords a further confirmation of the fact already established from the cases recorded by Lancisi, Morand, Morgagni, Portal, Corvisart, and Rostan, that rupture of the heart occurs much more frequently in the left than in the right ventricle. The latter observer attempts to explain this phenomenon by remarking, that most of the examples of this accident occur in persons affected with hypertrophy of the left ventricle, in which, while the sides of the ventricle become thick, the apex is extenuated, and by connecting this with another fact, that the laceration is almost invariably found near the apex, and on the anterior part of the ventricle. * The subject is curious, and deserves the attention of the pathologist; but requires confirmation from the evidence of subsequent cases.

Under the third head Mr Adams relates minutely four cases of contraction of the left auriculo-ventricular orifice, followed

* *Nouveau Journal de Medecine*, &c. Tome vii. p. 280.

by some interesting observations on the effects which this contraction produces on the circulation and its phenomena. These effects are first, the strong cardiac and weak arterial pulse; secondly, the irregular arterial pulse, and the occasional double cardiac beat, with the single stroke of the artery; thirdly, the venous pulse; and, fourthly, the tendency to apoplectic or epileptic death, to pulmonary hemorrhage, to jaundice, and to general dropsy.

The first of these phenomena is easily understood. The double cardiac beat, which Mr Adams thinks is only occasional, he regards as resulting not from a really double contraction, but from the failure of the arterial shock, owing to deficiency of blood to distend the canal, and communicate the cardiac impulse. The pulsation of the jugular veins is not uniform in all cases of contracted auriculo-ventricular opening. But when present, Mr Adams ascribes it to incapacity of the right ventricle to transmit the blood of the pulmonary artery. The varied terminations of this organic change are all to be ascribed to irregular distribution of blood to particular organs and sets of vessels, and demonstrate the influence of obstruction in deranging the circulation.

The essay is concluded with some observations on ossification of the aortic valves, and contraction of the arterial opening of the left ventricle, and a good case of ossification of the aortic valves, and of the coronary arteries, causing symptoms of angina pectoris, and death by debility of the heart.

The next article, entitled *observations on the origin and latent period of Fever* by Dr Marsh, presents a diligent and elaborate inquiry on a most difficult subject, the mode in which contagious fever is communicated from one individual to another, and the period which elapses between the first efficient exposure to the cause, and the distinct or manifest appearance of the symptoms. Dr Marsh relates twenty-two cases in which healthy persons, exposed to an atmosphere more or less charged with the effluvia of persons labouring under ordinary but severe fever, were at a certain time sensible of some loathsome impression on the external senses, quickly followed by more or less sickness, faintness, and languor, with sense of cold and shivering. The patients so affected were attacked in some instances soon after with the usual symptoms of fever; in others, after remaining for days or weeks indisposed, languid, and sickly.

The primary impression thus produced, Dr Marsh regards as the result of the operation of the animal poison, to which the name contagion is applied; and he is led from various considerations to infer that its operation is analogous, if not similar, to that of prussic acid, and the narcotic poisons. As in their case,

the first operation is on the nervous system, as evinced by depression of spirits, mental uneasiness, loss of muscular tone and vigour, disturbed sleep and uneasy dreams, rather than by any conspicuous symptom of disease. The secondary operation is, on the contrary, by a state in which reaction of the vascular system, more or less strong, as evinced by quick full pulse, heat, thirst, and deranged secretion, performs the principal part.

On the variable duration of the latent period, or that interval of time which elapses between the first exposure to contagion, and the manifest developement of its proper effects, the observations of Dr Marsh lead chiefly to this conclusion, that, if noticed, seasonable steps may be taken to prevent entirely, or at least to modify greatly the subsequent disease. No notice is taken of the valuable observations of Haygarth on this subject, which is to be regarded as an omission. Some good observations on the effect of cold in weakened states of the system, and on the means of obviating the effects of contagion, will be perused with interest and advantage.

In the last medical paper, the same physician gives a *brief notice of the effects of the vapour-bath in tetanus*. Of three cases in which this remedy, not higher than 90°, was tried along with or after others, recovery took place in two; and in one, though the disease terminated fatally, the tetanic symptoms had disappeared for more than thirty hours before death.

The papers on surgical subjects should now come under consideration. But this article has already extended to such a length, that the account of them must be deferred to a future number.

ART. III.—*A Treatise on the Nature and Cure of Rheumatism; with observations on Rheumatic Neuralgia, and on Spasmodic Neuralgia, or Tic Douloureux*. By CHARLES SCUDAMORE, M. D., F. R. S., &c. &c. London, 1827, 8vo, pp. 589.

RHEUMATISM is perhaps one of the most perplexing diseases which the practitioner has occasion to treat, and one in which the knowledge delivered in lectures and systematic treatises is least available. If the pathological principles and therapeutic precepts delivered in didactic discourses are too vague and general, those found in books are often unsettled and contradictory. One author agrees with Sauvages, Vogel, and Carmichael Smith, in placing the rheumatic action in the muscles; another thinks he finds it in the synovial sheaths; a third believes its erratic

motions can take place nowhere but in the aponeurotic membranes; and not a few will argue for the tendons and ligaments as its exclusive seat.

Nor is this dissent confined to the textures which the disease is supposed to occupy. One theorist contends that it is an inflammatory disorder, and advocates the indiscriminate and unsparing use of the lancet and all its accompaniments. Another, who maintains it consists in irritation only, proposes to lull the nerves with narcotics, and stupify the brain with opium. A third reasons with equal plausibility on loss of tone and impaired nervous power, and is confident that both may be restored by the tonic powers of Peruvian bark and the cold bath. If the old doctrine of acrid humour and biting rheums is ashamed to show face, a convenient substitute is found in the theory of diathesis, which explains every difficulty, and dissipates all obscurity, and proposes to change the whole train of unhealthy action by the mysterious influence of the alterative method.

Yet if speculation has been the parent of some of this discordance, it cannot be justly charged with producing all. In few diseases has the pure empirical method been more extensively tried; and in none can the results of experience be said to be so varied, so opposite, and, if superficially viewed, so irreconcilable. The blood-letting system, which is praised in Edinburgh, is reprobated in London, and unknown in Paris; and when the Edinburgh graduate quotes the authority of Pringle, and Cullen, and Gregory, or the practical lessons of the Clinical Ward and the New Town Dispensary, he may be opposed by a host of names, from Fothergill and Heberden down to Willan and Bateman, or reduced to silence by the ample experience of the hospitals of the metropolis. Guaiacum, which was found so efficacious in the hands of Dawson and Fowler, has failed to realize the character given it by these physicians; and though its employment is almost unknown amidst the growing reputation of meadow saffron and buchu leaves, they probably are destined to undergo a similar wain of popularity before remedies, which, if not more powerful, are at least more fashionable. When the efficacy of the bark is maintained on the authority of Hulse and Fothergill, and derives strong confirmation from the accurate inquiries of Haygarth, is it not denounced as highly improper by Cullen and Heberden, and stigmatized by Parry as perilous, if not pernicious in its effects? Lastly, who can adequately tell the true merits of mercury amidst the opposite characters given by different parties? Though found to be a remedy of general and permanent efficacy by Hamilton of Lynn Regis, by Curry, by Abernethy, and many others, yet Heberden, who thought it occasionally caused rheumatism, wished to

expunge it from the anti-rheumatic list ; and the sentiment which at present is spreading against its employment is too obvious to escape notice, and too general to be without effect.

These inconsistencies are a few of the evils of the art of healing ; and to the physician who is entering on the practice of his profession, it is of the utmost importance to see them reconciled. Their influence on the mind of any one is at once considerable and pernicious. They cannot fail to render his practice timid or unsettled, temporizing or contradictory ; and if he makes a bold effort to escape the misery of indecision, he is almost sure to plunge into unlimited empiricism. In no situation does the practitioner more frequently commit this error than in the treatment of rheumatic diseases. In no department of practical medicine could greater benefit result from judicious attempts to remove this source of embarrassment.

It may be too much to expect any individual author to be adequate to this arduous task. To accomplish it effectually requires not only most extensive practical experience, but unwearied patience in comparing its results with those of former observers, sagacity to distinguish, and judgment to appreciate shades of difference, and ready ingenuity to explain the contradictory and reconcile the inconsistent. It may not indeed be absolutely impossible for all these several qualities to be combined in one individual ; but their union is so rare an occurrence that ages may elapse before it is witnessed.

If, however, absolute perfection is rarely attainable, it is always useful to attempt an approximation. If it is impossible to raise the expectations of the reader by assuring him that the task now alluded to is achieved in the volume before us, he must be gratified to learn, that it presents a nearer approach to its accomplishment than any other work hitherto published. Dr Scudamore is already known to the profession as the most comprehensive and elaborate writer on gout. In the present work he has attained a distinction not much inferior on the subject of rheumatism. To his treatise on gout he had subjoined some brief observations on rheumatism, chiefly in contrast with the former disease. In the present treatise he attempts a full and comprehensive view of the history, pathology, and treatment of the several forms of rheumatic disorder. With much industry he has placed before the reader the most useful and precise information of the best authors on this disease, and digested into a single volume the collected results of many diligent inquirers.

It would be doing the author injustice, however, to represent his work as a mere compilation, however judicious. Though descriptions of well-known diseases cannot be expected to be ori-

ginal, those in the present volume bear marks of being at least improved from personal observation. The reasonings on the nature of rheumatism show that the author has observed much and reflected well, and his directions for treatment are manifestly derived from practical experience. The actual merits of the work, however, will be more easily understood from a short view of the manner in which the author treats his subject, than from any general character, however accurate and just.

Following the usual practice of dividing the disease into acute and chronic rheumatism, Dr Scudamore subdivides the former into acute and subacute, and traces the history, considers the pathology, and explains the treatment respectively adapted to each. His notion of the nature of the disease, so far as may be gathered from his definitions, and his reflections on its proximate cause, is somewhat peculiar, and not very definite. In the former he regards it not as an inflammation of any one texture, or even as an inflammation of a peculiar nature, but as pain of a peculiar character, with or without inflammatory action, (pp. 11, 12, and 13,) affecting many textures at the same time, but chiefly the white fibrous ones of the muscles and joints. Thus in acute rheumatism he conceives the morbid action to be seated in the ligaments, the tendons, the aponeurotic membranes, and the bursal membranes, but in the ligaments most frequently. In the subacute form, though any of these textures, and even the nerves may be affected, the disease is most frequently confined to the bursal, that is, the synovial texture surrounding the tendons. In the chronic form again, though the disease may occur in the ligaments and tendinous tissues, he represents it as most common in the sheaths of the tendons and the aponeurotic membranes.

Dr Carmichael Smith, it is well known, placed the rheumatic action exclusively in the muscular fibres. This hypothesis is not simply wrong in being too partial. Its author adduced no conclusive arguments in its support, and it stands in the situation of a gratuitous assumption. Dr Scudamore has satisfied himself by careful examination, by pressure of the whole course of a muscle, and grasping its substance during severe rheumatism, that the fleshy part is not the seat of the complaint. From the transitory nature of rheumatic disorder, from the immediate recovery of muscular action upon its cessation, and from the more permanent character of the symptoms when the substance of muscles is affected with disease, he is persuaded, that whatever inflammatory process seems to take place in that tissue during the presence of rheumatism is to be regarded as a remote effect, and not a primary form of the complaint. These inferences may be well founded. But it is not easy to reconcile

them with the opinion maintained by Pitcairn, Baillie, and other competent observers, that rheumatism affects the fleshy substance of the heart.

The further views of Dr Scudamore on the nature of rheumatic disorder may be understood from the following observations which he makes on the proximate cause of acute rheumatism.

“ It may be stated that rheumatism in its true primary character, as being a diseased condition of certain textures, is not so distinctly a constitutional disease as gout, which requires a specific state of constitution. If I may so express myself, rheumatism is a disease external to the system, which becomes affected in a secondary manner by sympathy; but in turn it reflects back its influence on the external parts.

“ Rheumatism, in common with gout, affects those textures which are not prone to the suppurative inflammation, as ligaments, bursal tendons, and nerves; but contiguous textures occasionally become affected to such a degree that pus is secreted by the vessels. I apprehend that in such instances, which are very rare, the cellular membrane, which is so universally interposed throughout the body, becomes secondarily affected in more than an usual degree. We see the hand swollen with much redness, arising from rheumatic inflammation, so that to the inexperienced eye the appearances would seem to threaten abscess, whilst of this result there might not be the smallest danger. This state of the integuments is secondary.

“ We are to keep in view the distinct and constitutional disposition of every specific inflammation. Rheumatic inflammation is not disposed to terminate in suppuration; but when it is active, fibrine (coagulable lymph) is often secreted by the inflamed vessels; and from the tendons and ligaments forming an adhesion to the integuments one species of ankylosis follows.

“ The remarkable manner whereby inflammation in rheumatism suddenly transfers itself from one part to another is peculiar to this disorder and gout; but since it possesses appropriate characters distinct from gout, and distinct from other diseases, we may, I conceive, with great propriety pronounce to be *specific*.”

So far as this is intelligible, it appears to be rather at variance with the notions conveyed in the definitions. In these the disease is represented to consist in *pain of a peculiar kind*; and inflammation, which is here admitted to be present, is said to be *usual* only, and therefore does not seem to be an essential constituent of the malady. If, indeed, we understand Dr Scudamore aright, he distinguishes rheumatism from rheumatic inflammation, in so far as the first is a primary affection peculiar, or at least confined, to certain tissues, while the second is a secondary result, which may take place in any other tissue, in consequence of the continuance or propagation of the proper rheu-

matic action. Thus he represents the tendons, ligaments, and aponeuroses, as the parts primarily affected by rheumatic disorder, and the affection of the bursal, the synovial membranes, the cartilages, the serous membranes, and the fibro-serous, as the dura mater, and the pericardium, as entirely secondary.

These opinions are so largely and ingeniously combined with matters of fact, that it is neither very necessary nor very easy to subject them to rigid scrutiny. Without taking into account all its minute points, the doctrine of Dr Scudamore may be said to consist of two leading articles, the universality, or at least the very general influence, and the specific nature of rheumatic disorder. And it must be conceded, in the first place, that these two conditions mutually corroborate each other, and that, if the one be admitted, the other must also follow as a necessary consequence. To us, however, it appears that both conditions, if examined attentively, will turn out to be little short of examples of the sophism termed *petitio principii*, and to rest on nothing but the unstable ground of hypothesis and assumption.

It is more than doubtful that rheumatism affects several textures at once; and if it really affects the tendons and ligaments primarily, which is very questionable, it can with less justice be said to affect the *bursæ mucosæ* than the muscles themselves. These sacs are synovial membranes placed between tendons and bones, or between tendons and tendons, and when inflamed are precisely in the same situation in which the synovial membranes of the joints are. Now, inflammation of the synovial tissues is totally different in phenomena from rheumatic disorders. Their form is not rheumatic, but more acute and more constant; and though the inflammatory process sometimes causes in them merely an abundant flow of their natural secretion, it too often produces albuminous exudation and adhesion, or suppuration, terminating either in granulation or ulceration. That this is the case is abundantly proved not only from the phenomena of diseased joints, but more especially from what is seen in that form of whitlow, which depends on inflammation of the synovial sheaths of the flexor tendons of the fingers. It is easy to see that, if the synovial membranes were affected in rheumatism in this manner, stiff joints would be much more frequent consequences of rheumatism than they are in reality.

That the ligaments are not affected it would be easy to show, if this does not lead into too long a digression. It may be sufficient to direct the attention to the following anatomical and pathological facts. Ligaments are not placed in situations in which rheumatic pains are most common and most severe. No part of the human body is more abundantly supplied with ligaments than the bones of the vertebral column; yet no part is

less frequently affected with rheumatism. In the extremities rheumatism does not appear where ligaments are situated, but follows the course of the aponeurotic coverings of the limb. Of the two sorts of ligaments employed in connecting the articular surfaces of bones, the extra-articular, and the intra-articular,—it is certain that the latter are *not* affected in genuine rheumatic disorder, and where the former seem to be the seat of pain, this is perhaps more justly to be referred to the incumbent or enveloping aponeurosis. Ligament is not very susceptible of primary inflammation; and unless perhaps in early life, or in consequence of violence, it is not the seat of secondary inflammation. In the most unequivocal case, when inflamed in consequence of injury, the process is slow, but severe; the ligament, or ligaments, scarcely recover their natural structure and powers, and in most cases they are completely destroyed.

If the tendons are inflamed in rheumatism, how happens it that they are not killed outright, as is seen in the inflammation of that species of whitlow above-mentioned, and in which they are very often thrown off in the shape of ash-coloured sloughs? In this disease, and in inflammation of tendons from wounds, bruises, and similar injuries, the phenomena of inflammation are very different from those rheumatic disorders. Inflammation of tendinous parts very often gives rise to tetanic symptoms, which never appear in the course of rheumatism.

The affection of the nerves, which Dr Scudamore also thinks constitutes a part of rheumatic disorders, appears to our apprehension the least tenable part of the hypothesis. When a nerve is inflamed it constitutes one disease, and when its neurilema is inflamed, another, both very different from rheumatism. The neuralgia, which has been termed *rheumatic* from a remote and improper analogy, is doubtless symptomatic of chronic inflammation of the neurilema. But this complaint may with equal justice be referred to gout or to any other disease of the whole nosology as to rheumatism.

These facts and arguments are here stated against the views delivered in the present work, not so much with the purpose of proving that they are entirely erroneous, as of showing that they labour under objections which, so far as we can judge, materially impair their general accuracy. It is quite possible that they may have occurred to Dr Scudamore, but that he did not think them of sufficient importance to invalidate the general truth of his theory. This, it must be admitted, is mere matter of opinion; and as neither one reader nor another will probably adopt implicitly all the conclusions of his author, it is not our intention to press this argument to any greater length, than

merely to remark what appear to be objections to the soundness of the theory.

Before quitting this part of the subject, however, we may remark that the objections now stated may be obviated by referring the rheumatic action to the aponeurotic tissue, or the membranes denominated *fasciæ*. The distribution of this membranous envelope is so extensive and so complicated, that, if its anatomical relations are well understood, it will not appear wonderful that rheumatism appears to affect muscles, tendons, ligaments, synovial membranes, and even nerves.

Thus there is scarcely a muscle of the human body which is not either covered or penetrated to a greater or less extent by aponeurotic membrane. Almost all the tendons, especially the flexor and extensor tendons of the extremities, are surrounded and supported by it. The synovial sheaths, or *bursæ mucosæ*, always derive from it an exterior covering. It is connected with the periosteum; it covers the ligaments; and it is stretched over the joints. In short, if the several divisions denominated *fasciæ* are attentively traced, they may be satisfactorily shown to be connected with each other so intimately as to form a great and general web extending over the whole body, supporting, enclosing, or penetrating muscles, covering and confining tendons, inclosing synovial sheaths, (*bursæ mucosæ*,) attached to the ligaments and periosteum, and covering the whole articular apparatus. It is obvious that, when the inflammatory process is developed in any point or points of a membrane so extensive, so complicated, and so varied in its anatomical relations, it may be propagated with great rapidity and facility from one extremity to another, and over a number of remote and separate divisions, between which it is impossible for any one unaware of its anatomical distribution to perceive any connexion. The peculiar pains of rheumatism are always most distinct in the situations where several folds of aponeurotic membrane meet, and their migrations may be generally traced along the course of its principal divisions. Even the kind of pain, as distinguished at least by its character of aggravated severity under the influence of external heat, seems to depend on the affection of the fibrous or aponeurotic tissue.

If these reasonings be just, the question regarding the specific nature of rheumatic inflammation is decided. For its peculiarity consists simply in being an inflammatory action of the aponeurotic tissue. Candour, however, compels us to advert to an objection of some weight even to this conclusion, and which appears at least to constitute a strong argument in favour of the specific nature of the rheumatic process. This disease is remarkable, like gout, in affecting not only external but inter-

nal parts. Thus rheumatism may affect the membranes of the brain, the pericardium, or the heart, or both, and even the intestines or their peritoneal coat. If it be said that the rheumatic action, when seated in the *dura mater* or capsular pericardium, is still in its proper seat of fibrous membranes, a view to which Dr Scudamore manifests an evident inclination, (p. 57 and 64,) this argument will not suffice in the case of the cardiac pericardium, of the heart, of the peritonæum, or of the intestines. If we admit the authority of Pitcairn and Baillie, we must believe that the muscular substance of the heart is liable to rheumatism. No doubt can be entertained that this disease sometimes causes severe or even fatal inflammation of the cardiac or serous division of the pericardium; (see this Number, p. 393,) and Mr E. Macdowell has recorded, in the first volume of the Dublin Hospital Reports, the fact of rheumatism causing fatal peritoneal inflammation.

It would greatly exceed the limits of these observations to enter into any inquiry into the reasons of this peculiarity in the natural history of rheumatic disorders; and perhaps a greater number of facts is requisite before any inquiry could afford satisfactory results. To view it as illustrative of what Dr Ferriar has called the *conversion of diseases*, states the fact in a different mode, but does not explain it; and unless the knot is cut by regarding the secondary affection as a new disease utterly different from the rheumatic, though produced by it, or by its causes, it is difficult to discover any alternative save that of attributing to rheumatism the property of creating a peculiar inflammation in one or more of several different animal tissues. It must not be forgotten, indeed, that Dr Scudamore undertakes to account for this peculiarity by considering the affection of the *dura mater* and *pericardium* from rheumatism, and also those of the synovial membranes and of the cartilages, as secondary or consecutive; but though this may answer the purpose of a nominal distinction, it does not afford an explanation of the phenomena.

In his observations on the treatment of acute rheumatism, Dr Scudamore enters into an elaborate and judicious examination of the effects, reputed and real, of different remedies and modes of treatment. To those acquainted with the principal practical writers on this disease, these observations may be less novel and interesting; but they are valuable to the young practitioner, in giving, at a single view, the experience of the most skilful physicians; and the critical remarks of the author must render them useful to readers of every description.

The propriety of blood-letting in acute rheumatism appears to be now generally admitted among all good practitioners; but there is still some difficulty in adjusting the extent to which

it may be carried, especially when, after two or three full blood-lettings, the rheumatic pains are inadequately relieved. Though examination of the beat of the heart by the stethoscope, as indicated by Laennec, will be usefully employed in obscure cases, Dr Scudamore thinks that in general the touch is sufficient to guide an attentive observer. The only character of the buffy coat to be relied on is its extremely cupped shape, and the great firmness of the clot beneath the fibrine. Upon the whole, though the general rule is to bleed while rheumatic symptoms and fever continue, exceptions must be made in particular cases, according to the age, the constitutional powers, and the habits of the patient; according to the season of the year, the state of atmosphere as to prevailing disease, and the topographical or physical peculiarities of the district.

The employment of purgatives in the treatment of acute rheumatism has been generally deemed inexpedient, from the aggravation of pain by motion, and the liability to aggravate the disease by exposure. Dr Scudamore, however, justly argues, that these objections may be obviated by attention and care, and ought not to deprive the practitioner of the advantage which never fails to result from the energetic and judicious use of purgative medicine. The formula which he has found to be most generally useful is a draught consisting of carbonate of magnesia, carbonate of potash, and sulphate of magnesia in small doses, tartarised antimony from one grain to one-eighth of a grain, vinegar of meadow saffron from half a drachm to one drachm and a-half, as much lemon juice as neutralises the carbonate of potash, with some palatable distilled water and syrup. This draught may be taken either during or after effervescence. He generally combines with it calomel and extract of colocynth at night, for the purpose of augmenting the excretion of bile; and in urgent cases the calomel is continued till the gums are slightly affected,

On the use of narcotics, and especially of opium in acute rheumatism, much difference of opinion has prevailed. Though Dr Scudamore regards its indiscriminate use as injudicious, and agrees with Cullen in condemning it, when inflammatory action is still high, and unless when given to procure sweating, he nevertheless accedes to the propriety of giving it at bed-time, and justly draws a line of distinction between opium alone, and opium combined with ipecacuan or antimony. In some severe cases of rheumatism it is indeed impossible to resist the administration of this drug, even in its pure form; and it is chiefly by putting the system under its influence that any sleep or respite from pain is procured. The expediency of this measure is demonstrated not only by the practice of Heberden, but by

the cases of De Roches, which, however, are not noticed by the present author. The best way of obviating the objections to its use, and of rendering it always safe and generally remedial, is by premising a full and copious blood-letting.

On the subject of Peruvian bark, Dr Scudamore gives the strong statements of Dr Haygarth due consideration; but he admits, that, even observing the rules laid down by this patient observer, he found that it very generally aggravated both fever and pain. The observations of the late Dr C. H. Parry of Bath on this ambiguous question, which, however, seem to be unknown to Dr S., are perhaps the most judicious that have been furnished by modern research, and may be safely referred to for the purpose of guiding the practitioner. But as these were on a former occasion placed before the readers of this Journal, it is unnecessary to recur to them at present.—(Vol. xxv. p. 385.)

The chapter on rheumatic metastasis contains, 1st, A condensed view of the papers of Sir David Dundas and Dr Wells on rheumatism affecting the heart and pericardium, followed by observations on the most approved method of treatment. 2d, Some remarks on rheumatic affections of the dura mater, and their treatment; and, 3dly, Extracts from the paper of Mr Wardrop in the Medico-Chirurgical Transactions, and that of Mr Mackenzie, in the Medical and Physical Journal, on rheumatic ophthalmia, and the appropriate treatment of this painful disorder.

Subacute rheumatism, the consideration of which forms the subject of the second division of the treatise of Dr Scudamore, is represented by him as either so partial or so mild as not to give rise to much constitutional disorder. The texture affected may be either synovial sheath, tendon, aponeurosis, ligament, or nerve; and in some instances one sort of tissue only is affected at the same time. Affection of the bursal membranes produces least, and affection of the ligaments most constitutional disorder. When appearing in the aponeurotic membranes, it may affect that of the temporal muscles, forming rheumatic locked-jaw, that of the cervical region producing crick-in-the-neck, that of the muscles of the lower jaw, or even its periosteum, constituting rheumatic toothach, that of the intercostal muscles causing *pleurodyne*, or spurious pleurisy, the lumbar fasciæ, when it constitutes *lumbago*, or the aponeurotic parts of the gluteal muscles when it causes genuine *sciatica*.

The treatment to which Dr Scudamore is most inclined is chiefly local. Blood-letting from the parts by cupping or leeches, or both, succeeded by evaporating lotions, or anodyne embrocations or plasters; blisters especially when the complaint is

seated over joints, as in the shoulder or knee; the soap-plaster, and mechanical support by means of bandages, afford material aid in removing the urgent symptoms. When constitutional treatment is required, the purgative draught formerly mentioned, senna, or the wine of meadow saffron, with a few grains of calcined magnesia, are all that the physician in general will have occasion to order. In that state of the disease which borders on chronic rheumatism, spirit of turpentine in doses of from twenty to sixty minims, combined with decoction of bark, Dr Scudamore has found to be a useful remedy. The warm and vapour bath, especially the latter, are also recommended; but Dr Scudamore does not attempt to define the sort of cases in which they are particularly indicated.

In treating of chronic rheumatism some valuable information is communicated on its consequences. The most usual of these is thickening and contraction of the *aponeuroses*, the tendons, the synovial sheaths, and the ligaments, producing more or less contraction and rigidity of the joints. In some instances even the periosteum may be thickened, and the cartilages may be covered with a morbid secretion. The remarks on the diagnosis of the several forms of this disorder merit particular attention.

The treatment of chronic rheumatism is attended with peculiar difficulty. The sudorific medicines which agree in one case, or set of cases, are unavailing in others; the tonic remedies under which the pains sometimes disappear, may be, and often are unavailing; and all our watering-places, hot, cold, and tepid, can readily attest both their occasional efficacy in certain forms of the complaint, and their complete failure in others.

Among sudorific remedies, Dr Scudamore seems to think that the election may safely be given to a combination of the volatile tincture of guaiacum, with acetate of ammonia and Dover's powder, or the vinous tincture of meadow saffron, with the two latter medicines. Mercurial alteratives he recommends when the complaint is complicated with disorder of the alimentary function, and a full mercurial course with the vapour bath in old and obstinate cases. When arsenic is thought expedient, he justly directs that it should be exhibited in union with some preparation of opium. Bark he has seen beneficial only when combined with guaiacum or oil of turpentine. Meadow saffron to be useful in chronic rheumatism must exercise its specific effects; and this is to be done by administering the wine of the roots, which should be given in doses of half a drachm twice or even three times daily, with a few grains of calcined magnesia, and from one to three minims of the black drop. If the wine of the roots irritate, that of the seeds is recommended;

and if this also disagree with the stomach, the vinegar, with a few grains of carbonate of potass or magnesia to neutralize the acid, or the evaporated extract, in the dose of one or two grains, may be given with good hopes of affording relief.

Sarsaparilla he represents to be useful in those cases in which the disease is accompanied with general derangement of health, without particular affection of any internal organ; the yellow flowered rhododendron where the disease depends on syphilitic taint; and the ling-liver oil in anomalous cases of obstinate rheumatic disorder.

Among topical application, leeches and cupping, with warm fomentations, are well suited to cases in which there is local pain and heat; and where there is much stiffness, stimulating epithemes, embrocations, and electricity are recommended. The observations on the merits of the different kinds of baths, natural and artificial, water, vapour, air, &c. are judicious; but we must pass from them to the subsequent parts of the work. It is to be remarked by the way, however, that the plan of treating rheumatism by sweating in body clothes, as practised by grooms in the cure of horses, and adopted with such singular success by Mr Chenevix, in his own case detailed in the Medico-Chirurgical Transactions, (vol. iii. p. 316,) receives too little consideration from Dr Scudamore, who mentions it only in his account of cases. As this method succeeded completely after the whole tribe of anti-rheumatic remedies had been unavailingly exhausted, there is every reason to believe that its remedial influence is great; and it would have been proper to have directed the attention of the reader more forcibly and immediately to a means of cure, which both reason and experience concur in recommending.

The last subject of the volume is *neuralgia*, rheumatic and spasmodic. To the first head is referred sciatica, which Dr Scudamore agrees with Cotunnus in considering as dependent on affection of the sciatic nerve or its neurilematic covering. This is perhaps too partial a view of its pathology. For though in a large proportion of cases the symptoms are distinctly referable to chronic inflammation of the neurilema of this nerve, in many cases, perhaps a still larger proportion, the disease consists in an affection of the gluteal aponeurotic membranes, passing in a greater or less degree to the fascia of the thigh. It would doubtless be a misapplication of terms to call this disorder *rheumatic neuralgia*. But though it has no title to the character of a neuralgic complaint, its dependence on affection of aponeurotic tissue gives it a clear title to that of a rheumatic one; and it is therefore to be viewed as the pure and simple form of genuine sciatica.

In considering the method of treatment, in addition to the

remedies enumerated under the head of chronic rheumatism, Dr Scudamore bears his testimony to the efficacy of the carbonate of iron, recommended by Mr Benjamin Hutcheson. The principal defect in this part of the work is a want of precise information as to the remedies most appropriate, and the order in which their use is most likely to be beneficial. In the treatment of a disease for which there are many applications, but few remedies, the great point is to simplify the method by the choice of a few judiciously chosen and energetic measures. Experience shows that the method most generally applicable to every form and stage of sciatica, is, *1st*, The detraction of blood from the part by cupping or leeches repeated at intervals of two or four days till the sense of pain is diminished; *2d*, The application of a blister twice or three times according to circumstances; *3dly*, The destruction of a portion of the skin by caustic or moxa, so as to produce permanent superficial inflammation; and, *4thly*, The employment of the hot-bath, the vapour-bath, or other means of applying heat, with the use of bandages in the intervals. With proper management of the diet and the digestive organs, most cases of sciatica will be much benefited, if not entirely cured under the diligent and persevering use of this plan of treatment.

Under the head of *spasmodic neuralgia*, Dr Scudamore examines the pathology and treatment of *Tic douloureux*. On the former subject he does not come to any very definite conclusion, and leaves the reader to form his own opinion from the varied results of histories and dissections. On the latter topic he recommends the usual remedies, and speaks in strong terms of approbation of the carbonate of iron.

This is but an imperfect account of the contents of the present volume; and can give only a very slight idea of the value of the information it contains. Dr Scudamore has illustrated his views and general conclusions by a copious but well-selected collection of cases, from which the reader may derive perhaps more benefit than from the generalized parts of the treatise. Whatever opinion be ascertained of the merits of the latter, it cannot be doubted that these cases, accompanied, as they sometimes are, with dissections and illustrative commentaries, communicate much valuable practical instruction, and must powerfully tend to correct whatever faults may have crept into the general descriptions and inferences. Upon the whole, though the present work is not to be viewed as perfect or faultless, it is unquestionably entitled to the character of being the best treatise on rheumatic diseases extant. By giving a just and comprehensive view of the present state of knowledge on the nature and treatment of these disorders, it supplies an important deficiency in the library of the practical physician.

ART. IV.—*Materia Indica ; or some account of those Articles which are employed by the Hindoos and other Eastern Nations in their Medicine, Arts, and Agriculture ; comprising also Formula, with Practical Observations, Names of Diseases in various Eastern Languages, &c. &c. &c.* By WHITELAW AINSLIE, M. D., M. R. A. L., Late of the Medical Staff of Southern India. Two Volumes, 8vo, pp. 654, and pp. 604. London, 1826.

THIS work was originally published in 1813 at Madras in the quarto form ; and a short character of its merits will be found in the twelfth volume of this Journal, (p. 347.) Of the present enlarged and altered edition little need be said beyond what may enable the reader to see that it is improved, and that its merits are sufficient to justify warm recommendation.

The great variety and interesting nature of the information contained in these volumes must indeed render them in all respects worthy of general attention. Every thing illustrative of the natural history of the various articles employed either in the treatment of disease, in the arts, or in agriculture among the nations of Asia, is collected from every accessible source, and rendered available to the English reader.

The first volume, which is also the first part, consists of three general divisions or chapters.

The first of these, which is by much the largest and most extensive, contains the history of the articles of the British *materia medica*, found or prepared in India and other Eastern countries, and of such articles of diet as are used in the management of the sick. In this history, which is arranged in alphabetical order, the description of the individual articles, whether derived from the vegetable, from the animal, or from the mineral kingdom, is given at sufficient length, and with great attention to scientific precision. Not only are the articles distinguished by their popular English names, and by those of the principal European languages ; but the synonymous appellations in the Tamul and Tellingoo, the Dekhanie and Persic, the Malay, the Cingalese, the Arabic, and even occasionally the Sanscrit are given ; and to the whole is added the scientific name in the Linnæan system. Though Dr Ainslie communicates whatever useful matter can be derived from the experience of the Hindoos, of the Tamul vytyians, and other native doctors, he has been at singular pains to collect the best and most recent European information on the chemical, pharmaceutical, and medical history of the substances employed.

In the second chapter, a similar account is given of the metals and metallic substances found in India and other countries; of their uses in the arts and agriculture; and of their employment in the practice of medicine in India, whether by European practitioners, or by the natives and their physicians. The alphabetical arrangement, according to the English nomenclature, is also observed in this chapter; and the foreign, oriental, and scientific synonymous appellations are added.

The third chapter consists of formulæ for the composition of some useful combinations of medicine, illustrated by practical observations on their employment in the cure of diseases.

To the second volume the reader is introduced, by some preliminary observations on the literature, science, and medicine of the Hindoos. Then follows the second part of the work, which, like the first, is subdivided into three general heads or chapters.

The first, which is the most important, contains a copious account of the substances used as medicines among Hindoos and other Eastern nations. The arrangement here is, as in the others, alphabetical; but it follows the order of the names of the substances in the Tamul language. The synonymous names in the Tellingoo, Dekhanie, and Sanscrit languages are generally, and the Linnæan appellation invariably annexed. The descriptions bear every mark of accuracy; and the natural dryness of scientific distinctions is much relieved by various interesting remarks and much amusing information, on the natural history of the substances, and their employment among the nations of India.

The second chapter contains a list of books in various Eastern languages on medicine and the collateral sciences, accompanied with occasional explanatory remarks. In the third, the author gives the names by which the greater part of diseases incident to the Asiatic nations are known in the principal languages of the East. Both chapters must be of particular utility to European physicians practising in India among the natives.

The nature of this work renders any detailed account of its contents at once impracticable and unnecessary. The accurate and judicious, yet simple manner, in which the information is conveyed, cannot fail to render it highly useful to every one who aims at extending his knowledge beyond the mere elements of medical science. It must be superfluous to say how great an acquisition it must be to those gentlemen whose professional services are required in our oriental colonial possessions. To them Dr Whitelaw Ainslie has rendered a most substantial service, by collecting into a treatise of moderate compass a great variety and extent of useful professional information.

It is not to be understood, nevertheless, that the present work is entirely faultless. The scrupulous reader will doubtless meet with occasional specks, which, however, are so few, or so trifling, that he thinks of them only to wish them expunged from a work in general of very great and equal merit.

Dr Ainslie is greatly too indulgent to Cape Madeira, (Vol. i. p. 476) which never deserves better than its ordinary reputation, of a meagre, carthy, and unpalatable wine. This is said on the ground of personal experience. But as Dr Ainslie may be inclined to regard the subject as a question of individual taste only, he may perhaps allow some greater weight to the observations of a competent judge, who, with every wish to speak favourably of Cape produce, found it impossible to bear testimony to the character of the wines of that colony.

“ Nearly all the wines that are made at the Cape,” says Mr Barrow, “ taste either very much of the fruit, or otherwise are meagre or sour. The first may generally be attributed to the must not having undergone a sufficient degree of fermentation to change its nature, but put up into pipes with much of the saccharine matter remaining undecomposed. The latter may probably be owing to the practice of pulling the grapes before they are ripe, in order to prevent their being consumed by the numerous tribes of insects that prey upon them, among which the common honey bee is not the least destructive.”

“ There is, notwithstanding every precaution that has hitherto been taken, a very peculiar taste in all the wines and brandies of the Cape, arising probably from the circumstance of the grapes growing so very near the ground. It is well known that the exhalations from the earth are so much imbibed by the leaves of the tobacco plant which grow nearest it, that those leaves are always rejected as unfit for use ; and it is natural to suppose that the fruit of the vine hanging very near to, or even resting on the ground,” (as it is suffered to do in most of the Cape vineyards), “ will also receive the prevailing flavour exhaling from the soil.”

Many passages to the same effect might be adduced from the same author. But these may suffice to show, that whatever be the cause or causes, the Cape wines, and especially the Madeira, are always of very inferior quality. The wine produced in the little farm of Constantia, has indeed acquired a reputation not inferior to that of the best European sweet wines. But this was owing to the diligence and enterprise with which its proprietor the elder *Cloete* cultivated this his favourite spot, and especially to the attention which he gave to the growth and culture of his vines. The success which attended the sale of this article has, nevertheless, tempted some of the wine growers of the Cape to bring into the market, under the name of Constantia, many wines of much inferior growth and flavour.

Under the head of KILANELLY (*Phylanthus Niruri*, Linnaeus), Dr Ainslie speaks of the *Chimaphilia umbellata* as a diuretic plant of the United States, mentioned by Dr Barton, and employed by Dr Somerville in the case of Sir James Craig. Whatever be the plant meant by Dr Barton, it is certain that the one employed by Dr Somerville in the case of the Baronet was not the *Chimaphilia umbellata*, but the *Pyrola umbellata* or umbelled winter-green. This is mentioned merely in passing; and perhaps Dr Ainslie will inquire into the matter, and correct in a subsequent edition what appears to be a mistake in the present.

ART. V.—*Elements of Physics, or Natural Philosophy, General and Medical, explained independently of Technical Mathematics.* By N. ARNOTT, M. D. of the Royal College of Physicians. London, Thomas and George Underwood, 1827. 8vo. Pp. 611.

THE application of the principles of mechanical philosophy, to explain the motions and functions of animal bodies, possesses the merit neither of novelty nor difficulty. But it requires some judgment to know when and to what extent it is applicable, and some discrimination to draw the boundary-line, beyond which it becomes at once futile and fallacious. The want of this knowledge brought the labours of the mechanical and mathematical physicians into such discredit, that their works are now rarely mentioned, unless to show the folly of their researches, and the extravagant length to which they were carried. Though possessed of all the learning and science of the day in which they lived, and endowed in general with talents quite equal, often superior, to those of their successors, they erred only in extending to all a mode of reasoning and inquiry, which should have been confined to certain branches only of physiological science.

Borelli had the merit of leaving one of the most brilliant monuments of the application of mechanical and mathematical philosophy to the science of physiology. But as neither his principles nor his applications were free from error, his example, which was followed by many ingenious men, led to numerous erroneous and fanciful speculations. His estimate of the impulsive force of the heart, though corrected after the experiments of Keill and Hales, and the calculations of Sauvages, is chiefly useful, as showing the inadequacy of the means to the end proposed. His hypothesis of the mechanical pressure of

the stomach has been long exploded, and is now mentioned only to expose the evil of precipitate generalization. His examination of the motion of the blood in the vessels, according to principles of hydraulics, may be in general just. But when we find Michellotti, Piteairn, and Bernouilli, applying them to the capillary circulation, and explaining the mystery of the different secretions according to the angles, the curvatures, and the sinuosities of the vessels, the figure of the pores, the shape of the globules, and the effects of friction; it is easy to see, that however accurate the reasonings may be, they are totally incompetent to afford correct results when applied to processes which are regulated, not by physical, but by vital properties, and which are too minute in their details, and under the influence of too many modifying agents, to admit of accurate measurement or nice calculation.

Notwithstanding these obvious objections, however, the founder of the hiatro-mathematical school had many rational followers, and nowhere more perhaps than in this country. To the calculations of Keill, and the experiments of Hales, the physiologist is indebted for many curious and valuable facts, and many original views on the phenomena of the circulation. The writings of Cheyne, though sophisticated with mathematical reasoning, show talent for pathological inquiry, and contain several new and original views on the nature of disease. Even in Jeremiah Wainwright's *mechanical account of the nonnaturals*, the discriminating reader may find some useful instruction; and if the writings of Bryan Robinson have not been otherwise profitable, they have furnished both facts and speculations to subsequent inquirers. The name of Mead will be remembered as long as the healing art continues to be the study of mankind; and the experimental inquiry of Clifton Wintringham is still entitled to the merit of directing the attention of physiologists to the physical properties of the animal tissues.

These men, ingenious as they were, carried their mechanical reasonings to an extravagant length indeed. Physiologists and physicians have since gone to the opposite extreme. The theory of vital properties and vital laws, first started by Stahl, then combined with the mechanical medicine by Hoffmann, and afterwards carried to the highest perfection in the hands of Haller, Whytt, and Monro, of Cullen, Hunter, and Bichat, was represented as alone admissible in the study of the actions, and the investigation of the functions of living animals. Reasoning founded on mechanical principles was represented as not only useless, but injurious,—as the parent of hypothesis, conjecture, and erroneous theory. To reason mathematically, or to apply

mechanical principles to the explanation of any of the phenomena of the animal body, was at once denounced as vain wisdom and false philosophy. To say that any thing was to be explained on mechanical principles, was a sure and ready method of taxing it with error and absurdity.

To such an extreme length was this unhappy antipathy to the hiatro-mathematical theory carried, that while it was accounted disgraceful in a physician to know any thing of mathematical science, it was stigmatized as the wildest and most fanciful speculation to apply it to physiology, and to imagine that the human body was under the influence of mechanical laws.

It is to be hoped that the profession are at length recovering from this hallucination. It is to be hoped that they can at length distinguish between the use and abuse of collateral science. Living bodies are doubtless endowed with peculiar properties, and are regulated by peculiar laws. And though these may sometimes oppose and not unfrequently control mere physical properties and laws, they do not entirely exclude their operation.

The impulsive force of the heart is an instance of pure mechanical power, operating in a certain manner and for a definite purpose. But this mechanical action is derived from vital properties, is influenced by the degree and condition of these properties, and in its exercise is liable to modification from many circumstances which obtain in animal bodies, but which have no influence in machines constructed of inert and inorganic matter. In the investigation of the action of this organ, therefore, the rational physiologist will not neglect the influence of one class of properties and overrate that of the other. It is his duty to observe the extent to which the operation of each takes place, the reciprocal influence which the one exerts on the other, the circumstances in which they mutually co-operate, and those in which the operation of the one excludes that of the other.

To take one more example. How much of the function of respiration depends on physical laws, and how much a knowledge of the latter contributes to the accurate comprehension of the former? Not to mention the chemical phenomena of respiration, which are of the utmost importance in the process, the physiologist should understand the nature and extent of the mechanical motions of the chest, the agents by which they are accomplished, the effects which these motions produce, first, on the lungs themselves, secondly, on the air contained in the breath-tubes, thirdly, on the blood in the vessels. He must further understand the nature, extent, and tendency of the motions of the larynx; the mode in which this organ operates in adjusting the quantity of air admitted, and the velocity with which it is in-

haled or expired ; and the important part which it performs in the mechanism of blowing or continued forcible expiration, of coughing, of sneezing, and of enabling the individual to fix the chest in extraordinary efforts of muscular exertion.

Illustrations of this description it would be easy to multiply did our limits permit. But these may suffice to prove the necessity of physical reasonings and principles even in the science of life and living bodies, and to show how little the physiologist can dispense with a knowledge of the elements at least of mechanical philosophy. Even if this knowledge were not susceptible of direct application in physiological inquiries, its indirect utility would still be considerable, by enabling its possessor to perceive where it was incapable of affording assistance, and showing where its guidance, if not fallacious, becomes at least unavailing. There are many situations in which the scientific inquirer discovers what is requisite and useful, by previously learning what is neither the one nor the other.

To communicate correct notions on this subject, and to unfold the application of physical to physiological science, the present work appears to be well calculated. In delivering a system of general physics or natural philosophy, the author undertakes to show their application not only to the arts, and to the explanation of mechanical contrivances in general, but to those branches of physiology which are under the influence of physical laws, and to such divisions of the healing art as are regulated by mechanical management. With the intention of rendering the treatise as intelligible as possible to general readers, the author has thought it expedient to divest it of all mathematical demonstration and reasoning, and delivers the general principles of each department of physical science with such proofs and illustrations as are mechanical and obvious to sensible observation only. This, no doubt, gives the work of Dr Arnott a less scientific appearance than it would otherwise have had ; and perhaps it is the source of some of that diffuseness and amplification which every reader will feel in a greater or less degree. But it must render the work so useful and popular, that it would be hypercritical indeed to make this a topic of censure.

The work is extensive ; and according to the division given by the author, the present volume forms little more than the half. Within its compass, however, he contrives to explain in three chapters the principles, 1st, of somatology and dynamics ; 2d, of mechanics ; and, 3dly, of hydrodynamics. The last chapter, which is the most extensive, is subdivided into four sections, in which the author treats the subjects of hydrostatics, pneumatics, hydraulics, and acoustics. Under each chapter or

section, the author arranges the illustrations afforded by the animal economy, and shows the application of physical and mechanical principles to elucidate the functions of the animal body.

Though every part of the volume is instructive, and none more so than those which tend to explain the phenomena of the material world, the extensive dominion which his knowledge has enabled man progressively to acquire, and the gradual improvement of the mechanical arts; yet it is chiefly in the physiological applications that the medical reader is interested, and to them principally that the present considerations must be restricted.

At the conclusion of the chapter on mechanics, Dr Arnott commences with the mechanism of the human skeleton, and considers the uses of its several parts, whether for the purpose of defence, of support, or of motion; and explains at some length the simple and compound motions of which they admit.

Under the head of *flexibility* is a lengthened discussion on the mechanism of the vertebral column, and a harangue, somewhat declamatory, on the popular subject of spinal diseases. The tone of it may be suited to the spirit of the times; but, if our opinion were asked, we should say that it is unbecoming the temperate dignity of science.

In speaking of dislocations and fractures, under the head of the bones of the extremities, the following criticism, if rightly understood, must cause some consideration.

“With our present information on these subjects, we are surprised at the detail of the practices and errors adopted in former times, and promulgated by authors of the highest credit, from ignorance of mechanics. It would hardly be believed that Mr Pott, one of the glories of English surgery, held and taught that, on reducing a dislocation of the shoulder or hip, it was useless to pull by the hand or foot, because the intervening joints prevented the strain from reaching the part desired.”

Now, we must be allowed to say, that the latter of these two paragraphs affords a singularly unfortunate illustration of the justice of the general inference conveyed in the former; and the only charitable mode in which we can explain their combination, is by supposing that the proper illustration had been mislaid; and that the present one had by some inadvertence or accident dropped in to supply its place. The modern reader may be surprised at the detail of some practices recommended for the reduction of dislocations, and may justly regard them as proofs of ignorance of mechanics. But so far is the instance here adduced by Dr Arnott from being regarded as an example of this ignorance, that instead of “hardly believing” it, we should

have been surprised if Mr Pott, glory or not glory, had taught any thing else than that it was useless to pull at the hand or the foot in reducing a displaced *humerus* or *femur*. Let Dr Arnott consider what is the effect of pulling at the hand or foot. Does the force so applied not move the hand from the wrist, and the foot from the ankle-joint, long before it makes any impression either on the fore-arm and shoulder or on the leg and thigh? If it does move the fore-arm or leg but a little, how can it produce much effect on the arm-bone or on the thigh-bone?

But waving all theoretical considerations, however sound and accurate, what is the actual fact in cases of luxated shoulder and thigh-bone? We have both reduced these and seen them reduced; and we can assert, without fear of contradiction, that pulling at the foot or hand is not only useless, but absolutely injurious, in so far as it causes much waste of force, and puts the patient to much useless torture. We regret to add, that it betrays not only ignorance of mechanics, but ignorance both of anatomy and surgery. We never saw a luxation of the shoulder or of the thigh reduced by pulling at the hand or at the foot. We never failed in reducing either, by applying the force to the arm or thigh respectively.

Dr Arnott is not more fortunate in a criticism which he subsequently delivers on the use of the pulley. Every mechanical contrivance is liable to abuse in the hands of those who are ignorant of the conditions under which it should be employed; and doubtless, if the extending force were applied to the foot, the power of the pulley would not only be exerted in vain, but in all probability would have the effect of causing instead of reducing a dislocation. Dr Arnott may rest assured, that the surgeon who understands the action of the muscles of the thigh-bone, who is aware of the effect of extension, and who also knows that this extension is most efficient when applied directly to the thigh, will use the pulley in reducing the bone when displaced, not only with perfect safety, but with much greater ease to his patient, and much less exertion to himself, than by any other method which could be devised. The practice of Sir Astley Cooper, to whom Dr Arnott cannot refuse the distinction of another "glory," is the most satisfactory commentary on the use of this mode of producing efficient extension.

The third chapter treats of hydrostatics, hydraulics, pneumatics and acoustics, in four separate sections; and in a fifth, the general principles explained in the previous sections are applied to elucidate the mechanical phenomena of the circulation of the blood, of respiration and voice, of digestion, and of such processes or actions as are carried on in the pelvis. To the medical reader this must constitute the most interesting part of

the volume; and we are happy to say that we think its perusal will afford instruction.

The mechanical phenomena of the circulation are considered by Dr Barry in the usual order, as they are manifested in the arteries, the capillaries, and the veins.

In considering the properties and functions of the first order of vessels, though he finds it impossible to doubt the existence of a contractile power in the arterial tunics, quite distinct from mere elasticity; yet he does not allow them to possess any proper or active contraction adequate to propel the blood which they convey; and he infers, that they contribute to its motion in no other mode than as rigid elastic tubes. He thinks, however, that, by enlarging or diminishing their calibre, they may influence the speed of its transmission and its distribution.

This appears to us to imply contradiction. If arteries do not contribute to propel the blood in any other way than as rigid elastic tubes, how can they be also said to be capable of enlarging and diminishing their calibre? If there is truth in physical denominations, it is the character of a rigid tube to continue at all times of the same dimensions. Conversely, if a tube can enlarge or diminish its calibre, it cannot consistently be said to be rigid. It is needless here to resume the debated question of the muscularity or the contractility of arterial tubes. It may be sufficient to say, that nothing proves the larger arteries to have innate powers of contraction; and so far as observation goes, they contribute to the progressive motion of the blood, which they convey, chiefly by the lateral resistance which their elastic walls afford to the column of blood urged onward by the uninterrupted action of the heart.

To the capillary vessels Dr Arnott ascribes much power. But he is not more successful than his predecessors in telling wherein this power consists. From the enumeration of the usual phenomena, a hundred times repeated, *e. g.* the blush, the momentary effect of various passions, &c. he infers the existence of a mechanical action, which he thinks is only to be ascribed to muscular or contractile coats. And to give this hypothesis its usual stability, he repeats the anatomical error, "that arterial branches have always more of the fibrous or contractile coat in proportion as they are smaller." It would be desirable to know in what *capillary* vessels this fibrous or contractile coat is seen; for most assuredly that cannot be considered as a capillary in which any thing like fibres is visible.

To the independent or innate action of the capillaries, whatever it be, Dr Arnott ascribes the vacuity of the arteries after death. This may be one, but it is certainly not the sole cause of this phenomenon. The early death of the left ventricle, so

clearly established by the experiments of Haller, and afterwards by those of Nysten and Le Gallois, is doubtless a circumstance of no ordinary power in contributing to this effect. Yet to this Dr Arnott never adverts; and adduces as an example of mistaken reasoning on this point, a most absurd opinion truly, and one which we never recollect to have met with save in the present pages. This opinion is simply, that the empty state of the arteries is occasioned "by the momentum with which the blood is supposed to be thrown from the heart in its last contraction, sufficient, according to them, to squirt it fairly through the most distant capillaries." It is not easy to understand to what authority Dr Arnott alludes. For we never met with a physiologist so hardy or so ignorant as to maintain an opinion so absurd, in the face of the well-known experiments of the authors already mentioned.

In the discussion of the circulation in the veins, Dr Arnott adduces the proofs that show the direction in which the blood flows into them from the arteries and capillaries; and inquires into the agents by which this progressive motion is effected. He opposes the opinion advanced by Bichat, and maintained on good ground, that the direct action of the heart ceases in the capillary system, and does not extend to the veins; and he argues for the power of the heart, the *vis a tergo* of the mechanical school, and that of the capillaries, in impelling the blood from the latter into the venous branches.

He admits, however, that the uniform current in the veins undergoes considerable disturbance in the neighbourhood of the heart from the three following causes:

1st, "As there is no valve between the veins and the auricle, each contraction tends to throw the blood back into the veins, as well as forward into the ventricle." 2d, "When the chest is expanded by inspiration, it is more roomy than during the collapse of expiration, and the blood enters it more readily." 3d, "While the chest is *inhaling* or *drawing* in air; that is to say, expanding so as to diminish the atmospheric pressure in it, it is by the same action favouring the entrance of blood by the veins towards the heart; and, on the contrary, while it is *exhaling* or *throwing out* air, it is with equal force resisting the entrance of blood, and slackening or even causing recoil of the inward current."—p. 504.

He then proceeds to inquire, whether any other agency than that of the heart, and of the capillary vessels, is necessary to accomplish the venous circulation, and whether any agency of this description is provided by nature. Inferring that there is none save the accessory and conjoined influence of the valves and muscular pressure, he proceeds to demolish the hypothesis of atmospheric pressure in the following manner.

"Some recent authors, as stated above, either not being aware of the facts which prove that the blood is every where pressed into the veins from the capillaries, with force much more than sufficient to raise it to the heart again; or being unable, from their little familiarity with physics, to draw exact conclusions from the facts, or to avoid errors in their own hypotheses, have promulgated the opinion that the progression of the blood in the veins is greatly owing to a suction power in the heart or chest; that is to say, to the atmospheric pressure remaining constant on the body generally, while it is occasionally lessened about the heart;—a circumstance, of which the whole effect, as stated above, is merely a slight disturbance of the uniformity of the venous current near the chest. Now such a doctrine could not be proposed or entertained for a moment by a person understanding the principle of a common household pump; and that it has been published and tolerated by able men in the present time, will remain a proof to posterity of the deficiency, as regards fundamental science or natural philosophy, which now exists in the ordinary medical education. Much ingenuity has been wasted upon it, particularly by Drs Carson and Barry, the latter of whom, after laborious investigations, by experiment on living animals, has even attempted to build upon it a superstructure of medical theory and practice. The fault, however, may be less in the parties who have been pursuing what appeared to them an important object, than in the system of education which left them exposed to such errors. Dr Barry need not blush to have proposed an explanation, in which members of the French Institute, sitting to judge it, were not prepared clearly to point out the fallacy. To say that the influence of the heart or chest is the power which draws the blood to the heart from the general system, is just as if one asserted that the ocean tide at the mouth of a river is the power which collects the tributary streams in the interior country."—pp. 506, 507.

With a view to the more full discussion of this question, he establishes the two following propositions:

"1st, The veins are pliant tubes free to collapse, and no pump can lift liquid through such. • 2d, The *suction* power of the chest in ordinary respiration is too weak to lift liquid a distance of even one inch through tubes of any kind."

"A practical illustration," he continues, "of the first proposition is afforded by putting the point of a syringe, capable of making a complete vacuum, if desired, into a piece of gut, or eel-skin, or vein filled with water, and then trying to pump up the water. The result will be, that the fluid close to the mouth of the syringe will enter it, and the sides of the pliant tube will then collapse as a valve against the syringe, making an end of the experiment. In exact proportion to the rigidity of the tube would be the distance to which the influence of the syringe would extend in it: if it required, for instance, half an ounce of pressure on the square inch of its surface to make it collapse, then the pump would draw up one inch of water, and so for other proportions. If during the action of the

syringe, the tube were allowed to open at the bottom into a vessel of water, instead of the syringe then drawing any more water from the vessel into the tube, the original contents of the tube would straightway be discharged downwards into the vessel: and the result would be the same even if there were a thousand tributary streams pouring into the tube, unless they entered with force enough to rise up to the syringe."—p. 508.

"Some bad reasoners on this subject have believed, that if a suction power exist equal to one inch column of liquid, any column, however long, must follow the first inch when acted on by the power in question; for, say they, the atmospheric pressure preventing a vacuum will prevent separation of the liquid. Now this reasoning is altogether inapplicable to pliant tubes, because the ready collapse of their sides will both allow the separation, and prevent the vacuum; and with respect to rigid tubes, it is equivalent to asserting that a force just capable of lifting one link of a chain, must therefore be able to lift any number of connected links."—p. 509.

"To illustrate the second proposition respecting the trifling suction power really residing in the chest, we shall state that a person of ordinary strength, using the power of the chest only, and not of the mouth separately, (which is a smaller and much more powerful pump than the chest,) cannot through a rigid tube suck water from more than about two feet below: and the opposite action of blowing outwards has nearly the same limit as is found by dipping the end of a tube two feet into water, and then trying to blow through it. Now, as water rises more than thirty feet towards a perfect vacuum, such as that of a good pump, the facts mentioned prove that the diminished pressure in the chest, as an approach to a vacuum, is never more than *one-fifteenth* of the whole, and that the increased pressure during straining, is in a corresponding degree. But in ordinary breathing, instead of differences corresponding to a liquid column of two feet or a *fifteenth*, the increase and diminution of air-density, in the chest, is measured by a column of less than one inch, or about a *five-hundredth*. This is easily shown on breathing through the nose, and holding one end of a glass tube in the mouth while the other end is immersed in water, by noting how much the water in the tube rises above the surrounding level during *inspiration*, and sinks below it during *expiration*. The mouth during this experiment may be considered as part of the general cavity of the chest, to and from which air is passing by the narrow openings of the nostrils. In tranquil breathing, with both nostrils open, the fluctuation in the tube is less than half an inch each way; with one nostril closed, and the other a little compressed, it may amount to a whole inch; and with hurried or convulsive breathing, like that of animal in terror or in pain, it may exceed twelve inches."—p. 511.

"In common healthy breathing, with the mouth open, the fluctuation of pressure in the chest is measured by less than half an inch motion each way of the liquid column. Dr Barry, not aware that this point could be so easily determined by the bloodless experiment described by the author above, or even by a simple calculation, has sought

the solution by numerous trials on living animals, into some part of whose chest he forced a tube; but even if farther experiments had been at all necessary, these of Dr B. could not have decided the question. 1st, Because the breathing became violent or unnatural, from the pain and agitation of the animals; and, 2d, Because the experimental tube often or always became a syphon; and Dr B. not adverting to this fact, has not recorded the difference of level in the liquid at the two ends. That the external level was for the most part higher than the internal, is proved by his having noticed almost solely the *inhaling* action of the chest, although the *exhaling* is generally an equal, and often a more powerful effort."—pp. 511, 512.

From these extracts our readers may be enabled to understand the train of reasoning pursued by Dr Arnott to demonstrate the insufficiency of the theory of Drs Carson and Barry, and to estimate the weight of the evidence adduced. When we gave an account of the experiments and deductions of the latter author, we endeavoured very much to make him speak for himself, leaving the merit of his opinions to be decided by their intrinsic soundness. At present we shall observe the same course with regard to the refutation here given by Dr Arnott. The question must be viewed as still open both to experimental inquiry and to rational discussion. Dr Barry, we must presume, is amply able to defend his own theory; and his ability will depend much upon its intrinsic truth. It is needless, therefore, in us either to investigate the justness of his views, or to adduce arguments in their support.

The subsequent part of the subsection consists of practical applications on the force of the heart, the velocity of the circulating blood, the pulse, and its modifications and varieties, the circulation in the head, and the effects of position on the motion of the blood in various parts of the body.

On the subjects of the mechanism of respiration and its morbid modifications; on that of voice and speech; on the mechanical actions concerned in the different processes of digestion, and especially on the formation of herniæ or intestinal protrusions; and lastly, on the various mechanical motions taking place in the pelvis or its organs, much curious and useful information is communicated. They can by no means, indeed, be said to be new: for they are simply what are found in the ordinary works, and in the best lectures on the mechanism of the animal motions. But they possess the advantage of directing the attention of the reader more immediately to the philosophical principles on which their operation is to be explained.

To conclude, the present volume, though stored with useful and agreeable instruction, and containing many things entitled to commendation, is, however, we grieve to say, by no means free from faults. Whether it be that the author is confident

in his knowledge of physical science, or, like some ingenious men, is prone to set the highest value on that with which he is best acquainted, we shall not stop to inquire. But the work is more or less pervaded with a triumphant spirit of the superiority of physical knowledge, and a proper contempt for whoever is deficient in it.

At one time the reader finds him censuring, in a severe and unsparing tone, the opinions or practices of some less gifted individuals. At another, he is found indulging in an ironical sneer, at the wisdom, the sagacity, or the ingenuity of some unhappy speculatists, who venture to think or act without possessing the golden key of physical science. He is, however, always ready to render the meed of honour to whom honour is due. But mechanical philosophy is the object of his unlimited adoration; and Smeaton, Rennie, and James Watt are the divinities of his idolatry. The Eddystone light-house is eulogised as the noblest triumph of mechanical skill. On rail-roads he expatiates with the most sanguine hopes of the perfectibility of mankind. And amidst the smoke and noise of the steam-engine he pronounces a glowing panegyric on the magical powers of that universal agent, and the miracles which it has enabled this country to achieve.

The style is generally clear, and almost always rather popular and attractive. But it is too florid and declamatory for a work of scientific instruction. The author's ideas appear to crowd on him, when he thinks of the advantages conferred by natural philosophy on mankind, and his language becomes then highly animated. But not unfrequently she hows want of taste, in allowing his energy to waste itself in mere words; and sometimes he betrays defect of judgment, in crowding too many particulars into a description, or pushing it further than the effect which he wishes to produce requires. Take for instance the following passage on the formation of waves, and the opposite extremes of embayed surf or a heaving ocean, and the glassy tranquillity of a sheltered sea.

"The common cause of waves is the friction of the wind upon the surface of the water. Little ridges or elevations first appear, which, by the continuance of the force, gradually increase, until they become the rolling mountains seen where the winds sweep over a great extent of water. The heaving of the Bay of Biscay, and still more that of the open ocean beyond the southern capes of America and Africa, is one extreme, and the stillness of the tropical seas, which are guarded by near encircling lands, is the other. In the vast archipelago of the east, where Borneo, and Java, and Sumatra lie, and the Molucca islands and the Phillipines, the sea is often fanned only by the land and sea-breezes, and is like a smooth bed, on which these islands seem to sleep in bliss—islands in which

the spice and perfume gardens of the world are embowered, and where the bird of paradise has its home, and the golden pheasant, and a hundred others of brilliant plumage; whose flight is among thickets so luxuriant, and scenery so picturesque, that European strangers find there the fairy land of their youthful dreams." p. 410.

No person who begins to read this passage, can have the most remote conception of the mode in which it is to terminate. Nor can any person after he has read it, trace the slightest connection between its commencing proposition and its concluding thoughts. With how much more propriety would the paragraph have ended, at the simple mention of the islands of the tropical seas?

Thus far had we written, when we perceived quite by accident, near the conclusion of the introductory discourse, various admissions of the possibility of want of "accuracy and polish;" and deprecations about omissions and inelegancies, with hopes that they "are not mixed with many considerable errors." This, we must admit, disarms our criticism, and commands forbearance. We are happy on the whole to say, that the work of Dr Arnott is not only free from considerable errors, but is a valuable acquisition to the medical reader, to whom we recommend its diligent and attentive perusal.

ART. VI.—*Notes on the Epidemic Cholera*. By R. H. KENNEDY, M.D. &c. Surgeon, Bombay Presidency. Printed at the Baptist Mission Press, Calcutta. 1827. 8vo, pp. 277.

WE have repeatedly had occasion to turn the attention of our readers to the deeply interesting subject of the cholera of the east. We shall now recur to it for a moment, that we may lay before them the additions made to our knowledge by the work of Dr Kennedy.

In an analysis given in this Journal for July 1825, of the reports of the three Presidencies, it was shown, that the Epidemic, Malignant, or Indian cholera, although it had never before been so alarmingly prevalent, or so generally diffused as it became in 1817, was by no means, as many imagined, unknown in India, or a disease of modern origin; and that Dr Scott, the compiler of the admirable report from Madras, had clearly traced a succession of partial visitations of it so far back as 1774, and even discovered descriptions of an analogous disorder in some old Hindoo medical writings. But Dr Kennedy has carried the researches of his predecessor still farther, and maintains that the epidemic cholera was very well known to Celsus, and is very well described by him. He alleges that modern readers allow them-

selves to be misled when they suppose that the Latin noun *bilis* is equivalent to the English term *bile*, whereas it was used to designate fluid secretions generally, and not the bile alone, which was always denoted by the noun *fel*,—that they have also erred in conceiving *cholera* to signify only a flow of bile, whereas it may with equal, if not greater reason, be translated a *flow from the intestines*;—and that no one would have thought of calling cholera, as it has lately appeared in India, a new disease, if the errors alluded to had not obscured the meaning of a passage in which Celsus describes clearly, though in his usual succinct method, an identical disorder. The essential parts of the passage referred to are as follows:—“*Primoque facienda est mentio cholerae; quia commune id stomachi atque intestinorum vitium videri potest; nam simul et dejectio et vomitus est: præterque hæc inflatio est, intestina torquentur, bilis supra infraque rumpit, primum aque similis, dein ut in ea recens caro lota esse videatur, interdum alba, nonnunquam nigra, vel varia. Non mirum est si subito quis moritur; neque tamen ulli morbo minori momento succurritur.*”—iv. 11.

Leaving these excursions into philology and ancient history, which are at least ingenious and novel, it is of more importance to remark that there seems little likelihood of the disease being soon lost sight of again. The Madras report, which was concluded in December 1822, notices several appearances of it in the course of that year, in the lower part of the Peninsula; and Dr Kennedy, whose information is the most recent, tells us that he saw it very prevalent at Baroda, in Guzeratte, during April and May, both in 1825 and 1826, and that he understood it raged about the same time throughout most of that province. In short, it appears highly probable that the epidemic cholera has never been entirely absent from Hindostan for the last nine years, and that it is now fairly established as one of the scourges of the country, of which a visitation in the epidemic form may be looked for at very short intervals.

Under these circumstances it becomes an object of paramount importance to determine as speedily as possible its origin and mode of propagation. In the notices formerly given in this Journal, repeated allusions have been made to an opinion, which seemed to be gaining ground among the practitioners of the east,—that it propagates itself by contagion or infection. To this opinion it would appear that many converts have been gained more recently; in 1825 it received the powerful support of Sir Gilbert Blane, founded chiefly on a consideration of the facts adduced in the reports of the presidencies; and now it is espoused in the most uncompromising manner by our author Dr Kennedy, who has added to Sir Gilbert's rea-

sons the results of an extensive personal experience. "To the best of my judgment," says he in winding up his chapter on this question, "I know no character belonging to any contagious disease which cholera does not appear to me to possess; and if it be not contagious, I know no other disease which I should be inclined to consider so." p. 67. It is chiefly to the question of its contagiousness, and to the no less puzzling question of its pathological cause or essence, that the observations of Dr Kennedy are confined; and it is to these subjects that we shall also confine the few remarks which follow.

And first of its contagiousness.—While we are ready to give Dr Kennedy ample credit for the sincerity of his opinion, the care with which he must have formed it, and a consciousness of "the delicacy of the ground on which he has advanced," we doubt whether the philosophic reader, who is acquainted with the late discussions on the doctrine of contagion in Europe, will be satisfied with the reasons which are adduced by the doctor, or will find that his exposition of them contains any new argument of consequence. While, therefore, we sincerely recommend this part of his work to the reader's regard, as containing a correct account of all that has hitherto been collected in favour of the contagiousness of the disease,—it will probably be more useful, if, instead of an analysis of Dr Kennedy's observations, we state briefly what are the most decisive facts to be looked for in the ulterior inquiry, which every one will on reflection perceive to be necessary for settling the present question.

1. It is quite unnecessary to attach any importance to the numerous instances recorded in which the disease, after appearing in a district, has extended itself over it apparently by communication with the sick. Facts of this kind are easily explained by the non-contagionists, provided it cannot be shown that the disease spread *gradually, and from the original spot of its appearance as a centre, except where a deviation from its regular course was connected with special inter-communication or special seclusion*. The argument derived from the extension of cholera among those who inter-communicate is nowhere stated, in this strong point of view, in any of the works we have had an opportunity of examining. We must not allow the opponents of contagion, however, to take advantage of this negative argument in support of their view of the question; for it is plain, that the facts in the particular shape we desire would not readily present themselves to any one spontaneously, or without the observer's attention being directly turned to them in the way of inquiry. It is singular, but not the less true, that of those who espouse the doctrine of the contagious nature of cholera, no one appears to have made himself master of the

recent European discussions on the subject of contagion, and, thus instructed, stepped on the field of practice to search for the means of decision.

2. The next argument in favour of propagation by contagion generally, though the necessary facts are much more easily collected, is not more strongly stated than the first in any of the Indian works on cholera. It is drawn from the circumstance that those who attend the sick are attacked in much greater proportion than others, who do not, but who in all other respects are circumstanced precisely in the same way; and an important addition is derived from the fact, that among the different classes of attendants the proportion seized is in the ratio of their attendance. The first part of this argument has been very well stated in a notice in this Journal (xxi. 488) regarding the propagation of the epidemic fever of Edinburgh in 1817-20 among the middle ranks. It may be more strongly expressed thus: Among the young men of the middle orders in the city fever was almost unknown, except among those who either attended the two hospitals where fever patients were admitted, or visited the out-patients of the dispensaries. Of the hospital students generally a few were attacked; of the dressers, who were longer in the wards, and in closer proximity with the sick, a much larger proportion suffered; and of the medical and surgical assistants, or *clerks* as they are called, who lived in the hospitals, not more than one escaped out of fifteen; yet the latter class were, with one or two exceptions, above the average in healthiness and strength. A concurrence of incidents like this compels the non-contagionist to resort to the far-fetched and improbable doctrine of the existence of local miasmata in particular houses, or even particular rooms. A similar argument may be constituted in various ways, but in no form is it so weighty as when drawn from the experience of a city hospital, because in no form is it more easily guarded from the various fallacies which surround it.

No satisfactory incident of this nature has been recorded in the publications on the cholera of Hindostan. It is stated by Dr Scott that thirteen medical officers of the Madras presidency died of it between 1817 and 1822; that fifteen or twenty more had it, but recovered; and that frequently the medical officer of an establishment was the only European who suffered. But so long as this statement is not protected against several obvious fallacies,—and particularly against the counter-statement, that the medical officers were in many instances the only persons of their rank who had occasion to visit the particular districts where the cholera atmosphere prevailed,—no valid inference can be founded on it. On the same account we must throw aside a

similar though somewhat better-stated fact, mentioned by Dr Kennedy,—that in the course of the twelvemonth ending June 1826, four medical officers of the Bombay establishment out of 116 died of cholera, while among the other gentlemen, military and civil, so great a proportion of casualties from cholera did not occur in the course of the whole prevalence of the epidemic from 1817 downwards. Several striking instances have occurred of its spreading among the nurses and other servants of hospitals. But so long as incidents of that nature are not guarded by a relation of the concomitant circumstances, little importance can be attached to them,—especially as those recorded are more than counterbalanced by other incidents directly the reverse.*

3. One of the most striking features of a contagious disease is its progressive advancement from district to district, and from country to country, and more especially the slowness with which it advances. Like Sir Gilbert Blane we can hardly see whence such a character can be derived, except from the mode of propagation being by human intercourse. When viewed in relation to this character, the history of the cholera of the east furnishes a very powerful argument in support of its contagious nature. So far as can be ascertained, it first appeared in the Company's territories about seventy miles north-east from Calcutta in August 1817. From this point it very slowly passed westwards across the base of the triangle forming the Peninsula, till in August it reached the coast at Bombay and Surat. At the same time it penetrated southward to the apex of the triangle by at least two distinct lines. One of these followed the coast from Calcutta, and it took from March 1818 till January 1819 to pass from Ganjam in the Northern Circar to Palamcottah, near Cape Comorin, a distance of 1000 miles. The other, leaving the middle of the transverse route between Calcutta and Bombay, penetrated southward to the apex of the triangle in two lines; and in passing from Nagpoor in Berar to Trivandrum near Cape Comorin, a distance of 900 miles, it took from May 1818 to January 1819. We might farther illustrate the present statement by referring to its very slow progress, occupying fully three years, from the north of India, through Persia and Syria, till it reached Antioch on the Mediterranean in June 1823, as stated in an analysis we have given

* "Every one," says one of the Bombay reporters, "of the medical attendants of the 65th regiment, and they are thirty in number, has been attacked." (Bombay Report, 46.) "In the hospital of the Royals, only one individual out of a hundred and one attendants was attacked." (Madras Report, 10.) Many other equally opposite facts might be quoted; but they are none of them material, as the reporters pay no regard to collateral circumstances.

of an historical document published by Dr Reimann of St Petersburg, (xxiii. 222.) But confining ourselves to what is completely and minutely authenticated, we repeat that its slow progress across and down the Peninsula in 1818 is hardly explicable, except by the doctrine of propagation by human intercourse.

This intercourse was established by means of the troops. And it seems there have been since 1817 circumstances connected with their movements, which did not exist before, and which will explain in part at least why the disease has passed so frequently from place to place since 1817, and why its visitations before that time were so partial. These circumstances have been touched upon with great felicity by Dr Kennedy.

“ Within the last forty years,” says he, “ the Peninsula has become but one empire ; and the communication which heretofore had been slow, casual, and hazardous, even betwixt contiguous districts, has thereby become not only uninterrupted and frequent in the intercourse of trade, *but is enforced from one end of India to the other, by the course of annual relief of troops* : a system of uninterrupted intercourse which, it ought to be remembered, was more particularly commenced on its present extensive scale in 1817, the first year of the epidemic.” p. 53. “ The breaking down of so many frontier barriers, and the consolidation of so many distinct and separate states (always jealous of each other, and frequently at open war) into one universal sovereignty, enjoying the most facile and uninterrupted intercourse, has, by the establishment of rapid and constant communication, invited or enforced between the extremities of the country, become the only apparent cause of the unusual dissemination, and regular succession of the pestilence, now under consideration.” p. 74.

No species of communication between district and district can be imagined more effectual for propagating a contagious disease than the movements of a body of troops, *particularly in India*. And that some such effectual intercourse is necessary to its propagation from district to district in the present instance, will, we think, appear from two particulars regarding the disease,—namely, the exceeding shortness of its course, and the brief interval which (if it really is propagated by infection or contagion,) elapses betwixt exposure and seizure. These two characters will not interfere with the rapidity of the diffusion of a contagious disease over a town or thickly-peopled country district, but must render nearly harmless all ordinary communication between one district and another even moderately remote. The cholera appears to have spread rapidly in a particular spot, but slowly from one part of the country to another.

4. By far the most unequivocal evidence of the propagation of a disease by intercourse with the sick is that which has been

so correctly and irresistibly enforced by Dr Alison in the present number of this Journal, in support of the contagious nature of the British fever, (see p. 241),—namely, the evidence of the disease breaking out in *several* previously unaffected districts, at a time corresponding with the arrival in them and sickening of persons who had intercourse with the sick in an infected district. It is to this criterion chiefly that we must look for the decision of the question of the contagiousness both of cholera and of other diseases whose propagation by intercourse is at present the subject of dispute. In the Indian reports we have been able to find but one instance of the kind; but as the report even in that instance is imperfect, and bears other intrinsic evidence that the compiler was not aware of its peculiar importance, we may be well assured that others may have occurred although they have not been noticed. At Jaulnah, which is 230 miles north-east-by-east from Bombay, and which had been previously free from the epidemic, it broke out on the 3d July 1818, a few days after the arrival of a detachment who left Nagpore while the disease prevailed, who suffered from it on the march, and *whose arrival was dreaded as their state was known*. From Jaulnah it appeared to pass by intercourse nearly about the same time to at least three, probably four other previously healthy stations, Aurungabad, 40 miles west, Malligaum, 100 miles west-north-west, Hyderabad, 250 miles south-east, and Nussurabad, 80 miles north-north-west. On the 6th July a party of gentlemen, with 1000 followers, who arrived in good health at Jaulnah three days before, set off for Aurungabad; on the way the disease appeared among them, and at Aurungabad it broke out for the first time a few days after their arrival. About the same time a considerable body of people left Jaulnah for Malligaum; on the way some of them were attacked, and on the 13th, soon after their arrival at the camp of Malligaum, which had previously been free from the disease, it broke out with violence. On the 4th July a brigade arrived in good health at Jaulnah, and on the 5th left it for Hyderabad; in a few days cholera appeared among them during their march, and it broke out *progressively* in all the villages on the way, which had been all previously exempt. Its appearance on the 14th at Nussurabad, which like the rest had been previously a healthy district, might with probability be also referred to communication with Jaulnah, as there was much intercourse between the two stations; but the Nussurabad report is silent as to any particular communication.—(Madras Report, *passim*.)

A frequent concurrence of incidents either on the large scale, like the foregoing, or on the small scale, like those mentioned by Dr Alison in the instance of the British fever, would consti-

tute presumptive evidence of contagion so strong, as not to be shaken by the numerous anomalous facts which have occurred to the medical gentlemen of Hindostan, and which, though apparently quite hostile to the doctrine of contagion, are so perhaps only because we are yet ignorant of the conditions requisite for its propagation. In investigating these anomalies it is proper to take into account a very judicious suggestion of Dr Kennedy, that it is probably incorrect to apply to the propagation of cholera all the rules deduced from that of the contagious diseases of Europe,—not so much, however, because the Indian cholera in his opinion belongs not to the class of fevers, but to that of nervous diseases,—as because it differs remarkably from the contagious diseases of Europe in two particulars, which must materially influence its propagation, namely, in the exceeding shortness of the attack, and the brief interval which, if it is propagated by intercourse, elapses between exposure and seizure.

5. Several curious instances have been noticed of the preservation of particular districts from the cholera by strict seclusion; but we are not disposed to attach importance to such facts under any circumstances, as they are open to many fallacies; and this objection applies with peculiar force to the instances alluded to, in which no account whatever is taken of these fallacies.

We cannot quit the present topic without remarking, that after all we are far from being satisfied that the cholera of the east is a contagious disorder, and see no sound reason for the unqualified opinion of Dr Kennedy. We insist, however, that a *primâ facie* case has been made out by him and others, which ought to lead their successors to more careful inquiry. If the numerous supporters of the opposite doctrine are inclined to find fault with us for placing their arguments in the back-ground, our defence is, that their arguments are in our eyes quite as unsatisfactory as those of their adversaries, if not more so; and that as a positive is always more easily established than a negative fact, and the positive side of the question is that upon which sanitary measures must in the meantime be taken, it appears to us more important to look towards that side of the question in endeavouring to point out by what means the truth is to be discovered.

We have left ourselves little room for noticing Dr Kennedy's exposition of his opinion respecting the pathological cause of this still mysterious disorder. The subject has been discussed at considerable length in our review of Dr Annesley's work, (xxv. 173 ;) on which occasion it appeared that the pathologist

was left very much in the dark,—that some supposed the disease to be nothing else than a variety of inflammation,—that others conceived it to depend on a peculiar nervous affection of the alimentary canal, without inquiring whence that affection originated,—and that others were inclined to think it arose from a peculiar state of the blood induced as an epidemic constitution, and tending when at its height to relieve itself by the evacuations which characterize this malady. Dr Kennedy advances a new doctrine, and states it explicitly,—namely, that “the collapse in cholera is induced by concussion of the brain produced by some unknown cause, and that the purging and vomiting are sanitary processes.” (p. 117.) The line of argument by which he supports this doctrine, and which is much too extended to receive justice from such an analysis as our limits will admit of, is shortly the following. There is no doubt, he says, that an affection of the brain may produce a state perfectly analogous to the general collapse which forms the first and sometimes the only stage in the epidemic cholera. Of this he has mentioned some very striking instances, which came under his own notice in persons of an epileptic tendency, and in which the fit seemed to be constituted by an attack of collapse alone. It is likewise illustrated very clearly in the first effects produced by the poisonous snakes, whose poison, he thinks, always operates on the brain in the first instance. And it is farther shown in the well known effects of fear, which in aggravated cases resemble entirely the collapse of the first stage of cholera. This state being once produced in the way alluded to, many facts, he maintains, tend to prove, that nature frequently attempts to relieve it by evacuations from the alimentary canal, which are sometimes excessive. The ordinary effects of fear, which we need not particularize, form one example in point; in some circumstances, (as in a very striking case which he witnessed of sudden and excessive fear, caused by a narrow escape from falling down from a high tower,) the successive phenomena are precisely those of cholera; and another example is occasionally furnished by the epileptic fit, which he has seen terminate in a profuse discharge from the stomach and intestines, sometimes like that of mclæna, at other times almost the same as that of the Indian cholera. A peculiar affection of the brain, therefore, analogous to that caused by violent fear, epilepsy, and concussion, and arising from the action of a morbid poison imbibed into the system, is, in Dr Kennedy’s opinion, the pathological cause of the epidemic cholera; and the evacuations which form the most striking feature of the disease in the generality of instances, and which have consequently led many to believe that the disorder is a local derangement of the stomach and intestines, are nothing more

than the result of the mode in which nature endeavours to relieve herself from the state of collapse which the affection of the brain induces. All we can be expected to say of this theory, is, whether it accounts for the phenomena, and leads naturally to what has been found the most effectual practice; and there can be no difficulty in allowing that in both respects his theory is a good one.

ART. VII.—*Pathological and Practical Observations on Spinal Diseases; Illustrated with Cases and Engravings. Also an Inquiry into the Origin and Cure of Distorted Limbs.*

By EDWARD HARRISON, M. D., F. R. A. S., Edin. &c. &c.
Thomas and George Underwood, London, 1827. 8vo, pp. 294.

OF this work it is by no means easy to say what opinion ought to be formed. The author indeed tells his story with sufficient plausibility. He describes the phenomena of the disease, which forms the subject of his inquiries; he introduces the anatomical history of the parts concerned; he reasons on its pathology; he investigates the merit of modes of treatment; he argues strenuously for that which he conceives the best; and he adduces cases with observations and very good engravings, to illustrate, exemplify, and confirm the whole. These several qualifications form the constituents of the bibliopoietic art. Whence then proceeds it, that of a book thus constructed, the merits are so ambiguous as to render doubtful its claims to attention or to neglect?

About seven years ago, that is to say in 1820 and 1821, Dr Harrison published in the *Medical and Physical Journal*, two essays on the subject of spinal distortion, in which he attempted to trace this complaint to pathological causes different from those to which it had been generally ascribed, and to show that the modes of treatment then in use were of no avail in arresting the progress of the deformity. These essays form the first, and a large part of the second chapter of the present volume, and constitute a sort of general introduction to the doctrines espoused by the author. As they have been so long before the profession, it is superfluous at present to make more than incidental mention of them; and as we must presume that their real merits are now sufficiently understood, it is unnecessary to enter into any formal examination of their contents.

The subsequent part of the work, which is entirely new, contains the further developement of the doctrines of the author, with such confirmation as is derived from cases treated accord-

ing to the method which he recommends. It is somewhat diffuse, and perhaps not arranged in such clear order as it might have been. Such as it is, however, the substance may very shortly be put before the reader.

Whoever is acquainted with the essays mentioned above, must be aware that Dr Harrison conceives the opinions of medical persons on the pathological causes of the several varieties of spinal distortion to be erroneous, and the methods of treatment founded on them to be inefficient, or even pernicious. Thus the notion of Mr Pott, he contends, is erroneous, at least in part, in so far as he represents *caries* or the affection of the bones to be uniform and essential to the disease. Dr Harrison argues, that as spinal deformity and paraplegia appear independently of caries, and as dissections show that the vertebral bones are not always enlarged or even diseased when accompanied with more or less deformity, the conclusion of Mr Pott is gratuitous.

The theory of treatment adopted by this celebrated surgeon, Dr Harrison considers as not less erroneous than that of the nature of the disorder. The object of Mr Pott, he says, was by inducing inflammation by means of caustic issues to effect ankylosis or union among the morbid bones. However useful this treatment may be at an advanced stage of the disease when the bones are really carious, Dr Harrison maintains that "it has never obtained the universal approbation of medical men;" and he regards it as highly objectionable at an early period, while the disease is still confined to ligament, because it prevents the application of other modes better calculated to restore the sufferer to his natural figure and former health. Dr Harrison, however, does not deny that recoveries have taken place under the method of Pott. But he ascribes them first to the stimulating powers of the caustic, which "encourage the muscles and ligaments to act more energetically," and sooner regain their lost tone and vigour; and, secondly, to the horizontal situation to which the soreness of these remedies necessarily confines the patient during the greater part of the treatment.

To the theory of softened bones proposed by Mr Baynton, if the term may be applied to that of which its author gives no very distinct views, and in support of which he adduces no definite reasons, Dr Harrison is not more indulgent. He is obliged, however, to admit that the confinement to the horizontal posture inculcated by this surgeon, with the view of removing the gravitating pressure of the superincumbent vertebræ and head, while the vertebræ are obtaining their proper hardness, must have much effect in preventing the progress of deformity, and promises to be advantageous in the management of these and other obsti-

nate complaints; but he seems to think it too simple, too tedious and inadequate even, when unaided by other means, and deficient in possessing no provision against relapses.

The third method of counteracting spinal deformity followed in this country is viewed in a still less favourable light. This, which consists in wearing what is called the *Hinckley instrument*, from the residence of Mr Chessher, its inventor, he justly regards as not only quite inadequate to prevent distortion, or remove the disease on which it depends, but as directly injurious by the great additional weight which it compels the unhappy suffer to sustain, and often aggravating the evil it was designed to alleviate. We willingly agree with Dr Harrison in regarding the use of Hinckley instrument as a very bad method of preventing spinal distortion; and in the few cases in which we see either it or any of its modifications employed, the consequences have been invariably prejudicial.

After this examination of theories and methods of practice, Dr Harrison quotes the opinions of Mr Wilson and of Mr Lloyd, to establish the general conclusion, that previous to 1821 little progress had been made in unfolding the true nature and improving the treatment of spinal distortion. At this period a new era, we are told, commenced; and a clear light began to shine on the obscure subject of spinal diseases. If not stated in express terms, it is at least pretty distinctly insinuated, that the source from which this illumination issued was the two essays already mentioned. If this be really the case, we sincerely hope that it may prove a genuine luminary, and not the fallacious glimmering of an *ignis fatuus*.

After demonstrating the insufficiency of former theories, Dr Harrison proceeds to show that the cause of evil in spinal deformity is in a very large proportion of cases to be sought in the ligaments; which, according to his opinion, become relaxed and elongated, and may suffer one or more vertebræ to be more or less displaced.

“According to this view of the subject,” says he, “we must direct our attention to some other tissue to discover the true cause of spinal complaints; and I am of opinion that we shall, for the most part, find it in the connecting ligaments, ‘which seem to have lost part of their power of holding the bones together.’* These get relaxed, and suffer a single vertebra to become slightly displaced. The column now losing its natural firmness, other bones are pressed unduly upon the surrounding ligaments; they in turn become relaxed and elongated, by which the dislocation is increased, and the distortion permanently established. The direction becomes lateral an-

* Pott, Vol. iii. p. 438.

terior or posterior, according to circumstances ; but the malady has in every instance, where the ligaments are the affected parts, the same origin, and requires the same mode of cure."—p. 38.

He then accounts for the different directions which the twist may take in the following manner :

" It appears to me, that when the spinal trunk," (this is not very intelligible,) " is too infirm to maintain the weight of the head and shoulders, the distortion inclines to the lateral form, being encouraged to take that direction from the resistance given by the superior spinal ligament. The anterior or internal curve, so far as my experience extends, has always been confined to the lower cervical and upper dorsal, or to the lumbar vertebræ. The spine is depressed in both instances, appearing to have sunk under and been crushed downward by an oppressive and overwhelming burden. When this arises in a weakly constitution, from the exertion of pulling, lifting or carrying, the protrusion from the force employed is nearly backward ; and thus is laid the foundation of the posterior or outward curve. This is the simple and real origin of our spinal deformities, except in some rare instances, where they begin in the bony or cartilaginous structure. On this principle, we can easily understand why people so often recover from spinal affections, and carry about with them the visible effects for many years."—p. 39.

It must not, however, be imagined that Dr Harrison ascribes all cases of spinal deformity to this relaxation and elongation of the ligaments. While he maintains that the greater number, according to his experience, have arisen from this as a primary cause, he admits, that in some instances disease originating in the bones, for example caries, deficient ossification, or preternatural softness, and in other instances disease of the fibro-cartilages may be the first and principal cause of a complaint which is to terminate in distortion of the spinal column. He therefore complains loudly that his doctrine is misunderstood by Dr Dods, who, it appears, had represented the opinion of Dr Harrison to be applied to all cases whatever of the disease, and had therefore censured it as too partial and exclusive. Whether this mistake was accidental or intentional, it is unnecessary to inquire. But from the language of Dr Harrison, it is extremely difficult to know the proportion or the kind of cases in which he conceives his pathological principles to be applicable.

Of a theory proposed in this manner, it becomes somewhat difficult to demand evidence ; and perhaps it may not be easy to agree on the kind and the degree of evidence requisite to establish its truth. Cases of spinal deformity, no doubt, come under necroscopic examination, in which it is impossible to recognize any trace of change of structure in the vertebræ or their connecting cartilages. But as the negative of one set of facts

does not prove the positive of another, so the absence of disorder of the bones and cartilages does not demonstrate that the ligaments are or have been diseased. The only satisfactory mode in which the opinions now advanced could be established, is by ascertaining from an extensive collection of cases of spinal deformity, the respective states of the different tissues of which the spinal column consists. Even after this was effected, it would be requisite to make corrections for cases in which it may be impossible to distinguish the primary from the secondary changes of structure ; and there is no method of accomplishing this, except that of observing those cases in which the disease is in various stages of progress.

Whether Dr Harrison is or is not aware of these requisites to establish the soundness of his opinions, he has not adopted any steps for attaining them. All that he appears to have thought incumbent on him was to adduce from the history of the anatomical and physical properties of the spinal ligaments, such considerations as might show their competency to the production of the effect, and to illustrate his positions by a few cases, which might or might not be held to be examples of distortion from this cause.

Dr Harrison sets out with observing, that fibrous structure, being incapable of stretching, is supposed to contribute in no respect to the morbid change of direction which the vertebral column is occasionally observed to undergo. This opinion, he contends, is erroneous for various reasons. 1st, When the neck of the thigh-bone is fractured within the capsular ligament, the shortening of the limb which takes place is attended with elongation of the capsular ligament, the tendons, and other fibrous tissues connected with the joint.—(p. 105.) 2. All yellow ligaments, as those connecting the vertebræ, are very elastic and extensible ; and, according to the conception of Dr Harrison, though strong, they are flexible, and admit of considerable motion, and, more than the white ligaments, “suffer the vertebræ to vacillate and recede.”—(p. 106.) 3. To these two circumstances is to be added the fact, that in different persons the bones composing the articulations admit of different degrees of motion ; and so great is this pliancy in some persons, that the spine can be bent and twisted with impunity in every direction.—(p. 107—111.) 4. The aponeurotic expansions and the periosteum, when exposed to the pressure of hard bodies beneath them, may give way and gradually enlarge.—(p. 111.)

Dr Harrison then gives a description of the articulation of the different parts of the spinal column, principally for the purpose of showing that its individual bones admit of such extensive motion, that they may be displaced in various degrees so as to constitute luxation, subluxation, or complete dislocation.

Here, further, he introduces, with the view of illustrating and confirming his opinions, various facts and arguments derived from the phenomena and anatomical characters of club-foot and other distortions incident to the extremities. In short, the conclusion of the whole train of argument and remark is to show that the ligaments are liable to become relaxed and elongated; and that in consequence of this they suffer the vertebrae to "vacillate," to "recede," and finally, to be more or less displaced. This may be said to be the whole substance both of the theory of Dr Harrison, and of the evidence on which it rests. To illustrate its practicability, and the method of treatment which its author deems most rational, nine cases are detailed at considerable length; and in one which terminated fatally from dropsy of the pericardium the state of the deformed spine is shortly described. These cases are, however, so deficient in many important particulars, such as the duration of the disease, its exact form, the remedies first employed without success, the time during which they were employed, and the time taken up in the alleged cure, according to the principles of Dr Harrison,—that we found it utterly impossible to give a general tabular view of their elements and result.

We have hitherto abstained from expressing any opinion of the general merits of these doctrines, not because we regard them as either just or faultless, but rather that our readers might have a distinct and correct view of their actual nature and tendency. This is more particularly requisite, after the misconception which it has been above noticed another writer on spinal diseases entertained; and if, in the wish to give a clear but compendious view of Dr Harrison's notions, we have failed to communicate a correct one, we trust the author will give us credit when we assure him that the failure is not intentional. We may also express the hope, that Dr H. will not ascribe to any other than a wish to promote the cause of true science, the remarks which we may have occasion to offer on certain defects to which, in our apprehension, his theory is open.

The substance of the present work of Dr Harrison may be said to comprise two objects;—one to show the extensive influence of the ligaments in producing spinal distortion, the other to establish the claim of its author to priority in proposing this theory. It is unfortunate that from this issues a singular dilemma, in which the theory of Dr Harrison is so awkwardly placed, that, so far as it is original, it is inadequate to explain the phenomena, and so far as it explains the phenomena, it ceases to be original.

Dr Harrison is perhaps not aware that he is not the first author who has advanced the opinion, that the pathological cause of vertebral distortion is to be sought for in the ligaments,

and that the distortion is caused by dislocation. If Dr Harrison will consult the twenty-third volume of this Journal, (p. 149,) he will find that this opinion, if not older, can be traced at least to Ambrose Paré; that it was also espoused by Bacheracht, and Monteggia, the eminent clinical surgeon of Milan. He is aware that Pott ascribed something to the ligaments; and upon his name, therefore, much stress need not be placed. Under these circumstances, not much, we fear, can be said about the mere originality of the theory. If this be an unpleasant discovery, it will not diminish the disappointment to find, that the theory had been abandoned from the conviction of its incompetency to answer the end proposed.

Dr Harrison has indeed attempted to give it strength and stability, by appeals to anatomical facts and pathological arguments. It is scarcely possible, however, to imagine a series of arguments more weak, more inconclusive, and less bearing on the decision of the point at issue, than those adduced by Dr Harrison. The whole of the argumentation amounts simply to this, that ligaments may be stretched or elongated, and that the bones may be displaced. Dr Harrison does not show that they *must* be stretched, or that they *are actually* relaxed and elongated; and as to the displacement of the bones, the reader must take that on his authority. Much less has he pointed out the circumstances in which they are elongated, or inquired into the causes of their relaxation and elongation, or adduced facts competent to prove that luxation *had* taken place, and that the ligaments *were* the agents by which it was effected. The whole process is so obscure and mysterious, that beyond mere relaxation and elongation, not a glimpse of light is obtained.

“The species to which I wish to draw attention,” says Dr Harrison, “makes its attack so insidiously, as not to be discovered till a long time after its invasion. It exhibits none of the local characters of inflammation, nor is it accompanied with pyrexia. This, which is the common variety, seizes upon relaxed fibres,” (that is, relaxation seizes on relaxed ligaments) “or persons accidentally debilitated. I am enabled, from the treatment of many such cases, to assert, that they are wholly unconnected with inflammatory diathesis, and very seldom originate in a morbid condition of the vertebral cartilages or bones.”—p. 46.

Now, to infer the absence of inflammation from the absence of fever and of the local characters of the process betrays a very limited and imperfect idea of this process. The inflammation may be deep-seated, as we shall show it is, and may cause neither redness nor heat. It may be chronic and insidious, and cause no great pain. It may be local, and is therefore unaccompanied with constitutional symptoms. It may nevertheless be in-

flammation, and that of a serious and unmanageable character. In short, the above argument is equivalent to saying that the ligaments are relaxed and elongated, because they are relaxed and elongated. The proposition is then an ultimate principle, and ought to be established as these are, by showing that it is a positive fact, beyond which it is impossible to advance.

Now, though the arguments of Dr Harrison can by no means be held adequate to prove that the ligaments are relaxed and elongated, and though it would be easy to show that they are not stretched to the extent which he imagines and assumes;—the question is not so much whether the ligaments are or are not elongated, as whether this elongation is the primary cause of the distortion. To this it may be confidently replied, that numerous facts show that it is not the primary cause. Take the very facts to which Dr Harrison appeals, and which he adduces as arguments in favour of this theory of elongation, and the true state of the matter will appear in a moment.

These facts are the cases of secondary or consecutive luxation, in consequence of disease of the articular apparatus. When the under-jaw slips out of its articular cavity, it is in consequence of the ligaments being stretched by repeated luxation, together with the readiness with which the muscles pull it over the margin of the articular depression, that the condyloid process is displaced from its situation. It is to be remembered also, that the structure of this joint is peculiar in having an interarticular cartilage, which in some persons is liable to enlargement, and thereby to thrust the condyle of the jaw downwards and forwards. When the head of the humerus drops out of the glenoid cavity, it is either in consequence of repeated luxations, during which, no doubt, the ligaments may be relaxed; or it is the effect of chronic inflammation attacking the synovial membrane and the cartilages, and by the swelling gradually extruding the head of the bone. A similar accident with regard to the head of the thigh-bone is no unfrequent result of disease of the hip-joint. In all these instances the part performed by the ligaments is entirely passive and secondary. The inference derived from the case of fracture of the neck of the thigh-bone within the ligament proves nothing, save the power of the muscles attached to the cylinder of the bone drawing it upwards against the pelvis.

The extensibility which Dr Harrison assigns to the ligaments, however real, tends in no respect to establish the position which he delivers. The assertion with which he commences his observations, that fibrous structure is supposed to be incapable of stretching, must be applicable only to some few ignorant persons, unacquainted with its anatomical properties. But though this quality is admitted to exist in fibrous or ligamentous

structure, it is only under certain conditions, and invariably as a passive property only. For the sake of proving this to be correct, it is unnecessary to go further than the work of Bichat, in which Dr Harrison will find the extensibility of fibrous tissue (Vol. ii. p. 161) represented in its true light, and considered under proper limitations. He will there find, that from various facts, experimental and pathological, extensibility of fibrous tissue is a secondary effect only; that it is called into place by the active operation of some other agent; and that it is possessed in so slight a degree by this tissue, that laceration or rupture is by no means an unfrequent consequence of the gradual but irresistible operation of a distending force. The application of this to the theory of Dr Harrison is so obvious, that it may be left to the discretion of the reader.

The pliancy of joints, and the extensive motion possessed by some persons, which Dr Harrison has adduced in support of his opinion, cannot with justice be applied to demonstrate the tendency to incurvation in the spine. Pliancy in the joints of the extremities may depend on the formation of the articulating ends of the bones, on the elasticity of the cartilages, on the energy of the muscles, and sometimes on the slow progress of ossification. These circumstances do not admit of application to the motions of the vertebral joints with the same facility, or to the same extent as in the joints of the extremities. The structure, configuration, and connexion of the former, present to their motions anatomical limits, which are not found in the case of the latter. In the case of the spinal column the direction of the oblique articular processes, and the union of the spinous processes, render any extent of motion beyond a certain limit anatomically impossible; and it is easy to prove, that, unless in very early life, when the process of ossification is still incomplete, the length and firmness of the spinous processes must form a great obstacle to much extent of motion of the spinal column.

Here also must be noticed a peculiarity in the pathological principles of Dr Harrison, to which it is impossible to attach the weight which he seems to claim for it. He argues strenuously for the fact of the *vertebræ* being subluxated, luxated, or even entirely dislocated. Now, every one who understands the articulations of the spine knows that this is, in the ordinary acceptation of these terms, an anatomical impossibility. In other words, unless the connecting tissues are entirely rent asunder, dislocation or even luxation is out of the question. In this, however, Dr Harrison is a most implicit believer; and consistently enough, no doubt, he talks of restoring the displaced bones to their natural situation. Even supposing it possible for such luxations to occur, as he imagines to have occurred in

his cases, we cannot understand how the application of strips of adhesive plaster, or the manipulation practised by him, can replace a vertebra so dislocated;—much less can we perceive how the spine can be stretched, and the vertebræ pressed or driven in with impunity.

It is superfluous to say, that we do not wish to be thus understood to deny the possibility of what may be denominated *consecutive luxation* of the spine. But this accident, which is distinguished as being the consequence of previous disease of some of the component parts, is widely different from that for the existence of which Dr Harrison contends. This is indeed the sort of displacement which takes place in incurvation, and other species of spinal distortion. But even this never takes place to the extent imagined by the present author. As an understanding of this may tend not only to rectify some of the notions advanced by Dr Harrison, but also to explain the general mechanism of vertebral deformity, we shall shortly state what we conceive to be the correct view of the matter.

The vertebræ are mutually connected in two modes; first, by mere conjunction; and secondly, by articulation. The conjunctions are at three different places; 1st, at the bodies by the intervertebral fibro-cartilages; 2d, at the spinous plates by means of ligament; and, 3dly, at the spinous processes by means of the yellow ligaments, (*ligamenta flava*.) The articulate mode of connexion is much more limited, and is confined solely to what are termed the oblique processes. It is the inflammation of the synovial membrane of the latter, acute, sub-acute, or more frequently chronic, which is very generally the primary cause of spinal distortion. The synovial membrane thickens, the cartilage swells, and the oblique process of the one vertebra is separated from that of the other. The effect of this, aided by the tendency which the individual shows to throw the weight of the body to the opposite side of the spine, produces not only a lateral bend or incurvation, but, in consequence of the peculiar shape of the sound spine, a considerable twist or rotation. It is this also which produces the elongation of the ligaments, on which Dr Harrison dwells at so much length. For the swelling of the synovial membrane and of the cartilages necessarily stretches the capsular ligament and the neighbouring connecting tissues, as these processes do in other articulations.

It is not unimportant to observe, that, according to the direction which this disease takes, it causes a lateral bend, or an antero-posterior incurvation. First, it may be, and often is confined to the upper and lower articular surfaces of the oblique process of one vertebra, and to the lower surface of the one

above, and the upper surface of the one below, affecting in this manner parts of three vertebræ on the same side. Thus is produced the lateral bend and rotatory twist. Instead of this, however, it may affect both oblique processes of the same vertebra, and by mutual spreading towards the middle injure seriously the spinous plates and processes of the bone. As this rarely continues long without affecting the vertebra above and that below, three bones will also in this case eventually be diseased; and before the complaint has proceeded far, a considerable posterior incurvation will have taken place.

The anterior incurvation is caused by a similar process taking place, either in the fibro-cartilages, and thence passing to the vertebræ, or in the vertebræ themselves, and proceeding to the cartilages. This is in all probability the disease so much dwelt on by Mr Pott.

Another source of spinal deformity peculiar to the dorsal vertebræ remains to be noticed. These bones are provided not only with the usual oblique process of articulation, but also with an articular notch for the half of the head of the intermediate rib, and an articular surface on the transverse process for the tubercle of the superior rib. This complicated apparatus renders the dorsal vertebræ peculiarly liable to disease, and consequent distortion. Not only may their oblique processes be affected, so as to produce all the evils already enumerated, but the notches at the sides of their bodies, and the articular surfaces of the transverse processes, are particularly obnoxious to inflammation of a strumous character. The tendency is too obvious to escape observation, and may be readily understood from what has been said above.

A third source of spinal distortion differing from either of the two now mentioned, in being more general in nature and origin, should not be omitted. In some persons the process of ossification is so tardy that the whole osseous system, and more especially that of the spine, continues unusually soft, and fails to acquire the natural firmness and density till a late period. This imperfect ossification may vary in different individuals; but we have remarked it so late as the twentieth, the twenty-second, and even the twenty-fifth year. In such circumstances, one or more of the imperfect vertebræ are very liable to give way; and as scarcely one can yield without deranging the relative position of the rest, more or less distortion is very easily produced; and when once produced it is very readily increased.

We have not had means of ascertaining the proportion of cases in which this cause of distortion takes place; but several circumstances tend to show that it is by no means unfrequent. More than once we have seen two young persons in the same

family continue apparently straight and well-shaped till the same period of life, and both become much and manifestly distorted from the same cause.

These desultory remarks are offered rather as suggestions for observers who have leisure and opportunity to apply them, than as a complete view of the pathology of vertebral diseases. The limits within which this hasty sketch must be restricted render it impossible to enter minutely either into proofs or illustrations. They will in general, however, be found to correspond closely with what is disclosed by dissection of the several varieties of spinal disease. The most obvious conclusion to which they lead is the fallacy of supposing that elongation of the ligaments is the primary cause of spinal distortion. When the ligaments are thus elongated, they are so in consequence of previous disease of one or more of the contiguous tissues. In almost every case it will be found that the synovial membrane of the capsule, or its synovial glands, or the cartilage which it covers, is the part originally diseased, whether that disease is afterwards propagated to cartilage, to ligament, to bone, or to periosteum.

We have dwelt so largely on the pathology, that but little space is left for considering treatment. Already have we adverted incidentally to that practised by Dr Harrison, and recommended in strong terms as eminently efficacious. The supine posture, or at least frequent recumbence, is doubtless indispensable in all incipient disorders of the spinal column; and in all likelihood this is the best part of the treatment. Of the stretching the spine, the pressing and driving in prominent vertebrae, and restoring displaced ones to "their native beds," we cannot speak from experience; and we must repeat the avowal of our inability to understand its operation, or appreciate its merits. The strips of adhesive plaster appear to be insignificant, unless as the means of mechanical support. So far as good theory would lead, while the stretching, pressure, and driving in must be condemned, the recumbent posture, and the slight mechanical support might be retained with advantage; but their influence would be much increased by the early employment of means for counteracting local inflammation. The repeating detraction of blood from the parts where the deformity first appears, by means of leeches and cupping-glasses, afterwards one or two applications of a blister, and even the use of the caustic, cruel as it appears, afford much more likely means of counteracting spinal disease, and arresting the progress of spinal deformity, than the mechanical means so confidently recommended by the present author. Dr Harrison is mistaken when he imagines that the operation of the caustic

is to promote ankylosis and consolidate carious bone. Its first and only effect is by long-continued superficial inflammation to counteract that which is deep-seated. It is perhaps the most powerful and permanent antiphlogistic which the surgeon possesses, and should not be decried or abandoned on slight grounds.

ART. VIII.—*Review of some of the Surgical Cases which have lately occurred in the Royal Infirmary of Edinburgh, —A Clinical Lecture delivered to the Students of Surgery in that Institution, on Thursday 26th July 1827.* By GEORGE BALLINGALL, M. D. F. R. S. E. &c. &c. 4to. Pp. 23.

THE present short publication, which is a continuation after the model of that published in February, and mentioned in our April number, is, for several reasons, entitled to some notice.

Dr Ballingall has a very just idea of the nature and uses of hospital practice, and of the proper objects of hospital reports; and a short statement of the results of different modes of treatment, executed in the manner he proposes in the introduction, could not fail to be interesting, and in the highest degree instructive. Though Dr Ballingall has found it at present impracticable to carry this plan into effect, it may be hoped that at some future period he may be more successful in realizing his views.

In the present lecture the cases are important in impressing on the mind of the student several curious points of pathology and practice, and in forming a summary of the principal facts which had fallen under observation during the course. It is not easy to say what is the best, for it is all useful, and must be perused with advantage. As a specimen of the nature of the subjects discussed, the following account of the case of an idiot, well known in Edinburgh, may be selected as one which excited much interest, and was in some respects extraordinary.

“ This poor lad was admitted on the 6th of June with an oblong elastic tumour under the integuments of the left groin, following apparently the course of the spermatic cord, and bearing somewhat the resemblance of an inguinal hernia. This tumour was slightly discoloured on the surface, and was stated to have originated suddenly in consequence of a kick from a jack-ass. I had little doubt that the great bulk of the swelling was formed by the effusion of blood; it was treated in the first instance by leeching and cold lotions, under which the inflammatory appearance on its surface

diminished, and for some time I had hopes that it would be removed by absorption. But the tumour having become stationary, I opened it on the 23d of June, and turned out a large quantity of coagulated blood from the upper part of the swelling; in the lower part of it the effused blood had become to all appearance partially organized; it presented somewhat the appearance of the fibrinous deposit found in aneurismal sacs, and on attempting to remove it, fresh bleeding was excited. The wound for some time promised to suppurate kindly, and the remains of the coagulum were likely to be thrown out in the form of a slough; the lad's health, however, became impaired, frequent oozings of blood occurred, and the matter in the bottom of the wound assumed rather the appearance of a soft fungus than of a slough. On the 3d of this month the wound was enlarged downwards to the bottom of the scrotum, the whole of the fungoid mass which could be readily detached was removed, and the oozing of blood which followed was restrained by the application of hot turpentine, which was continued for several days, with the double purpose of suppressing the bleeding and hastening the detachment of any remaining portions of slough. But the patient's health continued to fail; the fungus again sprung up, and was not now confined to the bottom of the ulcer from which it originally seemed to arise, but a fresh growth of the same description took place from the upper point of the ulceration, which had extended towards the anterior and superior spinous process of the ilium, and here the excrescence obviously originated from the integuments or cellular membrane, and not from any deeper-seated part. Various escharotics were applied without effect, and on the 16th an attempt, which was of necessity a forcible one, was made to destroy the fungi with the scalpel and actual cautery; but this was counteracted by the patient's violent struggles, and little or no ground was gained. On the 20th he submitted patiently to the application of ligatures round the roots of the fungi, and on the following morning the lower one appeared somewhat shrunk, and less vascular; but he has since obstinately resisted any attempt to tighten the ligatures. He has now become exceedingly irritable, timid, and suspicious. Within these few days we are threatened with a fresh protrusion of fungus from the upper part of the ulcer, while the former ones continue to increase. His pulse is now 120; his body much emaciated; and his appetite greatly impaired; so that there is little or no room to hope for amendment.

"From the view which I first took of this case, I represented it to you as a variety of hamatocele, as a bloody tumour lying over the course of the spermatic cord, but not contained within its sheath. You will recollect that before it was opened the great bulk of the tumour lay high up in the groin, above the ring of the external oblique, and that while this part of it was discoloured and livid, the scrotum on that side presented its usual appearance, slightly corrugated on its surface, free from tenderness or discoloration, with a firm globular body to be felt at its lower and back part, which was conceived to be the testicle. It was obvious that the testicle had

not descended into the scrotum on the right side ; and it is now positively asserted by a person who has been in the habit of visiting this patient, and who formerly slept with him, that he never had any testicle on either side of the scrotum.

“ From the subsequent progress and present state of this case, it is impossible to speak with precision as to the anatomical structure of the parts. The lower part of the fungus had at one time very much the appearance of those fungi which are not unfrequently found to spring from the testicle, but in this instance the upper part of the excrescence, at least, had certainly no such origin. In speaking of the vessels from which the blood was in the first instance effused, and by which the fungus was subsequently nourished, I mentioned the epigastric, the circumflex iliac, and the spermatic, as those most probably engaged ; and I noticed several anomalies in the distribution of these vessels, observed by different anatomists, some of which may possibly exist here.

“ In the treatment of this case the state of the poor creature's mind has been all along a most formidable obstacle, by throwing additional obscurity on the nature of the disease, and by rendering him incapable of mustering sufficient resolution to submit to the most probable means of relief. You have all seen how inefficacious the employment of escharotics has proved ; and the attempt which you saw me make on the 16th to reduce the excrescence, by shaving off a portion of its surface, and afterwards cauterizing it, was, I think, rather prejudicial than otherwise. It was evident that the patient's violent struggles produced a determination to the seat of the disease ; every successive writhe of his body propelled an additional quantity of blood into the tumour, part of which escaped, and part lodged in its texture, and led to its increase.”—pp. 13, 14, 15.

At the time when the lecture of Dr Ballingall was delivered, the report of the case was continued no further. By the kindness of the author, however, we are enabled to give the sequel, illustrated by the account of the appearances after death.

“ On the 27th of July, the day subsequent to the last notice of the case, a considerable oozing of blood occurred from the surface of the tumour, which had now attained the size of the largest lemon. In my absence the application of ice was directed by Dr Cullen, and this was followed by some diminution in the bulk of the fungus ; it became soft and flaccid, lost all appearance of vitality, and seemed disposed to sphacelate. The patient's strength continued also to sink, and he expired on the morning of the 30th.

“ Previous to examining the state of the parts concerned in the disease, the cavity of the abdomen was opened, and a quantity of injection thrown into the descending aorta. The upper portion of the fungus, that part of it which has been described as springing from the integuments, dropped off without any force being used to separate it ; the lower part of it was attached to what was no doubt the remains of the testicle, although the gland was so completely disorganized as to render it impossible to demonstrate its structure.

The spermatic artery, originating from the aorta, and equal in size to the epigastric, was traced, along with the *vas deferens*, through the inguinal canal; and coupling this circumstance with what was felt in the scrotum in the early stages of the case, and the absence of any testicle on the left side of the abdomen, I can entertain no doubt that the testicle on that side had been in the scrotum previous to the injury, notwithstanding the assertion of the patient's visitor, and the opinion of some of my professional friends to the contrary.

"On the right side the testicle was found within the abdomen, occupying the situation which it is known to do in the *fœtus*; although smaller than an adult testicle, it was quite healthy in its structure, and was furnished with a *vas deferens* of the usual size. This vessel followed nearly the course of the ureter, and was traced down into the pelvis, where the seminal vesicles were found of equal size, quite natural in appearance, and on both sides distended with fluid. The abdominal ring or opening in the external oblique was very small, resembling that of the female, and a few delicate blood-vessels were found passing through it; but although the parts on this side were carefully dissected, nothing like a chord or cremaster muscle could be traced.

"The foregoing case, as far as I know, is altogether unique. The state of the patient's intellect; the source of the injury and the consequences which followed; the rapid growth of the fungus, and the size which it attained; the singular extension of this fungosity to the structures contiguous to the testicle; the contradictory statements as to the existence of a testicle on the left side of the scrotum; and the remarkable appearances met with on dissection, form altogether a concatenation of circumstances probably never met with before, nor likely to occur again."

There are several other cases more important perhaps in a practical point of view. But for these the reader must consult the tract of Dr Ballingall itself. That of Hugh Morison, from whose cheek a large hard tumour with the characters of scirrhus sarcoma was removed with favourable termination, is particularly deserving the attention of the student of surgery.

PART III.

MEDICAL INTELLIGENCE.

MEDICAL CHEMISTRY.

On the Composition of the Black Vomit thrown up in certain Diseases of the Stomach.—(*Journ. de Chim. Méd. Juillet 1827.*)—In our last number we gave an account of the composition of the black matter thrown up by vomiting in a case of ulceration of the stomach. Its chief peculiarities were the presence of caseic acid, and of melanosed blood. M. Collard de Martigny has analyzed a similar fluid discharged in a case of chronic disease of the stomach, and has discovered in it the same principles. He also procured instead of the substance intermediate between mucus and albumen, which was found by Las-aig-ne, another principle, analogous to gelatin, but more soluble in the cold, and destitute of the property of forming a jelly.

M. Collard de Martigny winds up his analysis with some judicious observations on the utility of organic chemistry in the diagnosis of diseases. Many examples are now well known of certain diseases being invariably accompanied with certain changes in the secretions. These changes are sometimes cognizable by the external senses; sometimes they require the manipulations of the chemist before they can be ascertained; and the progress of pathological chemistry is daily adding to the latter list. It is correctly remarked by our author, that organic principles are frequently found in diseased states of the body, which are never found in its healthy state,—that certain principles seem invariably to be produced during peculiar states of disease,—and that of the diseases attended with discharges, there are probably few in which some peculiar principle may not be detected. It is impossible to foresee the extent to which organic chemistry may be made serviceable in the strictly practical department of the healing art; but enough has been done even already to show that the physician who neglects it neglects one of the most precise, and, we may even venture to add, simple means of diagnosis which modern science has offered to him.

Relative Strength of the different Wines of France.—(*Journal de Chim. Méd. Juillet 1827.*)—The following proportion has been assigned by M. Julia Fontenelle as indicating the relative quantity of spirit in the wines most generally used in France. The numbers indicate the per centage of alcohol at 19° of the areometer, and are the mean terms of many experiments:

Banyuls,	-	-	21.96	Béziers,	-	-	-	18.40
Rivesaltés,	-	-	21.80	Lunel,	-	-	-	18.10
Colliouvre,	-	-	21.62	Montpellier,	-	-	-	17.65
Lapalme,	-	-	20.93	Carcassonne,	-	-	-	17.22
Sigan,	-	-	20.56	Frontignan,	-	-	-	16.90
Mirepeisset,	-	-	20.45	Bourgogne,	-	-	-	14.75
Salces,	-	-	20.43	Bordeaux, (1st quality)	-	-	-	16.90
Narbonne,	-	-	19.90	Do. (2d quality)	-	-	-	14.70
Lezignan,	-	-	19.46	Do. (<i>ordinaire</i>)	-	-	-	12.70
Leucate and Fitou,	-	-	19.70	Champagne, (still)	-	-	-	14.00
Montagnac,	-	-	19.70	Do. (sparkling)	-	-	-	12.02
Nissan,	-	-	18.80	Toulouse,	-	-	-	11.97
Mèze,	-	-	13.60					

M. Fontenelle's results for Lunel and Burgundy differ considerably from those obtained by Mr Brande, who for the former assigned the proportion of 15.52, and for the latter 12.79. M. Fontenelle never found either so weak, and supposes that Mr Brande had operated on new wines, which he observed to be invariably much poorer in spirit than those which have stood a few years.

ANATOMY AND PHYSIOLOGY.

On the Reproduction of the Crystalline Lens. (*Journal de Physiologie, Janvier 1827.*)—It is generally believed that the lens of the eye, being one of the solids of the body, is not reproduced when it has been removed by operation. But recent experiments made by MM. Cocteau and Leroy-d'Etiolle prove that it is frequently reproduced at least in the healthy state of the eye. They performed the operation of extraction on many rabbits, cats, and dogs; and they found that in most instances, though not in all, the capsule examined at the end of four or six weeks contained a new body of a lenticular form, and approaching in consistence to that of the extracted lens. In one of their experiments they allowed the animal, a rabbit, to live till six months after the operation. "The crystalline capsules were then found perfectly transparent, without a visible cicatrix; and they contained each a lens of the same volume and consistence as those extracted. For the sake of greater certainty they were immersed in hot water, when they became opaque, hard, and friable, like ordinary lenses, the sole difference being, that the disposition in brilliant plates was evident only on the outer layers." The circumstances which determine their reproduction are not very apparent. Perhaps it is prevented by the same causes which occasion cataract; for certainly in persons operated on for that disease the lens has been often found not reproduced.

Cerebro-Spinal Fluid. (*Journal de Physiologie, Janvier 1827.*)—Various opinions have been at different times entertained as to the existence or non-existence of a fluid in the cavities of the skull and spine; but at length the general idea seems to have been, that it does not exist in a state of health, and is either a morbid production, or the consequence of exhalation after death. M. Magendie, however, has lately made many observations on man and animals, and has arrived at an opposite conclusion,—namely, that in a state of health there is always more or less fluid between the pia mater and arachnoid membranes of the brain and spinal column. It is found along the whole course of the spine, over the surface of the brain, and in the ventricles, with which the fluid on the external surface of the brain communicates by an opening two or three lines wide, opposite the *calamus scriptorius* of the fourth ventricle, and between the posterior cerebellar arteries. It passes easily from one region to another, particularly from the spine into the ventricles of the brain; during the action of nodding, during expiration, during violent muscular action, or when a tight bandage or other pressure is applied to the abdomen, it is impelled towards the ventricles; during the contrary actions it returns into the spine. Its composition is peculiar: according to Lassaigne, it consists of ninety-eight parts of water and nearly two of solid matter, the solid ingredients being composed chiefly of osmazome, with a little albumen, muriate, and carbonate of soda, and phosphate of lime. Its principal use seems to be to preserve the brain and spinal marrow in a state of uniform compression; for if it be removed in a living animal, dullness, listlessness, and inaction are caused; and if it is augmented by the fluid taken from another animal coma and apoplexy ensue. Hence when any change takes place in the volume of the encephalon, its functions are preserved and maintained by a converse change in the cerebral fluid. If an effusion of blood occurs, the quantity of the cerebral fluid is rapidly lessened by absorption; on the contrary, when the brain diminishes in volume, as in old age, the quantity of fluid is very much increased, both on the outside of the brain and in the ventricles. Some degree of pressure is exerted by this fluid on the organs it covers; for when the vertebral *theca* is punctured, a jet several inches in height is formed; and it is probable that the pressure varies with circumstances. No other fluid can serve the same purposes: even pure distilled water when substituted for it induces immediately a state of general convulsions, and other fluids increase that effect. It appears to be constantly undergoing absorption and renovation: when a solution of the ferro-cyanate of potassa was injected into the pleura, it was detected ten minutes afterwards in the cerebro-spinal fluid, and in twenty-four hours it had disappeared again. It like-

wise seems to pass from one part to another with great facility : on exposing the opening of the *aqueductus sylvii* into the third ventricle of the brain, it will be seen that slight pressure on the sacral end of the spinal sheath causes the flow of the fluid into the ventricle. It further appears to be in a state of continual motion from place to place. This is shown in a very striking manner by the following pathological facts. A woman died of apoplexy in a few hours, in consequence of an extravasation of blood into the third ventricle : the surface of the clot was found blanched, and the whole cerebro-spinal fluid was tinged with blood. In another similar case, in which the clot had got into the aqueduct of sylvius and dilated it, but without any part having escaped from the fourth ventricle, the coloration of the spinal fluid was very deep. In a case of inflammation of the pia mater of the spinal chord, ending in suppuration, and proving suddenly fatal while the patient was trying to get out of bed, pus was found in the four ventricles of the brain, without any appearance of softening of their parietes, or vascularity of their lining membrane. In a case of apoplexy proving fatal in thirty hours, and caused by a large clot in the substance of the left hemisphere, there was no communication between the clot and the ventricles, yet the whole cerebro-spinal fluid was deeply coloured with blood ; and it turned out, on minute examination, that the clot communicated by a small laceration of the brain with its external surface. These cases prove that the cerebro-spinal fluid is every where in communication, and constantly in a state of motion from place to place ; and they show how symptoms of disease may exist in one part of the cerebro-spinal apparatus, when the active cause of the disease really exists in another place. On this subject M. Magendie promises a more detailed inquiry.

PRACTICE OF MEDICINE.

On the Treatment of Acute Rheumatism by Tight Bandaging.—[*Archives Gén. de Médecine*, Juin, 1827.]—In the number of this Journal for October and January last, a short account was given of a proposal made by M. Velpeau of Paris, to treat the several varieties of diffuse subcutaneous inflammation by graduated pressure, applied by means of the roller bandage. The same plan has been lately tried with great success by Dr Varlez, surgeon-in-chief of the military hospital of Brussels, in the treatment of acute rheumatism of the joints. He has related several cases in which the bandage was applied in the acutest form and stage of the disease, when the redness and swelling were great, the throbbing pain violent, and the general fever high, and in which the treatment was crowned with speedy and complete success. In almost every instance the application of the bandage is followed at first, and for two or three hours, with increase of the pain, so that the patient can with difficulty be persuaded to persevere ; but soon afterwards the pain begins to abate, and the abatement then advances with rapidity. Dr Varlez generally moistened the bandages with an emollient decoction, which, besides having the ordinary effect of emollients, were useful by increasing the pressure.

On the Treatment of Inflammation of the Tongue. (*Horn's Archiv. für Medizinische Erfahrung.*)—The following cases of this rare but alarming disease form a useful appendix to the paper of Mr Martin in our last number, and will show that the severe but effectual practice adopted by him may sometimes be superseded by milder treatment. *Case I.*—A stout man, 68 years of age, by trade a mason, was attacked with pain in the throat, redness of the fauces, and swelling of the tonsils, but without much fever. These symptoms were considerably diminished by leeches applied to the angles of the jaws, emollient gargles, and a mustard pediluvium. But in consequence of exposure to cold he was soon afterwards seized during the night with a tormenting sense of tightness at the root of the tongue, difficulty in speaking and swallowing, and ere long with swelling in the tongue and impending suffocation. When his physician saw him in the morning he could not speak, the pulse was very quick and feeble, the countenance pale and anxious, and the tongue filled the whole cavity of the mouth. On account of the firmness with which the tongue was pressed against the roof of the mouth, and the patient's inability to separate the jaws to half the usual distance, it was found impossible to make on its upper surface more than two short incisions, from which very little blood flowed. An incision was therefore made with the lancet into each sublingual vein ; and when the blood ceased to flow, fresh incisions were made, and the discharge promoted by injecting warm water into

the mouth. In the course of two hours the swelling was so much diminished that the man was able to rinse the mouth with warm milk, after which a blister was applied to the neck, and a purgative enema administered. A rapid amendment then took place, so that before the close of the day he could speak intelligibly; and his recovery went on steadily. *Case II.*—Another case was of a less acute form than the former. It had existed eight days before it came under treatment. The patient had little pain or fever, but swallowed with great difficulty, and could breathe only in the upright posture. The case was cured by blisters, gargles, and the mustard pediluvium.

Employment of Iodine in the Treatment of Cynanche Parotidæa. (*Rust's Magazin für die gesammte Heilkunde*, 1826.)—Dr Neumann of Neustadt in Silesia has employed the hydriodate of potass with great success as an external application in cynanche parotidæa, which prevailed epidemically in Neustadt in June 1823. Among the poorer orders who were treated in the ordinary way, the disease was very tedious, and generally ended in suppuration. Among the better ranks, the treatment consisted in the exhibition of an emetic, and the application over the tumour of a plaster composed of eight parts of mercurial ointment and one part of the hydriodate of potass; and the ordinary effect was a radical cure within three or four days. Dr Neumann adds, that he never observed in the cases so treated any metastasis to other organs; and he is disposed to impute this to the formation of an erythematic eruption, which always appeared on the first or second day, and remained from eight to twelve days.—The author has annexed a very instructive case illustrating the deleterious effects of this drug when given in too great quantity. It was given to cure enlargement of the cervical glands, which it accomplished very effectually; but the patient was at the same time attacked with violent palpitation in the chest and abdomen, which was relieved only by the horizontal posture, then with great weakness, and tendency to faint, and subsequently with emaciation and general dropsy. The palpitation was so violent and constant as at first to appear connected with organic derangement; but the relief obtained from the horizontal posture, the absence of cough and dyspnœa before the dropsy commenced, and the regularity of the pulse, led Dr Neumann to conceive that the disorder of the heart was merely functional. Accordingly, although the patient had been ill for a twelvemonth, the irregularity of the heart's action was soon subdued by the employment of digitalis and cherry-laurel-water; and the tendency to faint and anxiety at the same time disappeared. The dropsy, however, continued much longer; but at length it slowly receded under the use of the *Arnica* combined with Soap.

LEGAL MEDICINE.

Poisoning with Monkshood.—(*Journal de Chim. Méd.* Juillet, 1827.)—A family in the neighbourhood of Lille were poisoned by this plant, in consequence of its roots having been mistaken for those of the *Ligusticum livisticum*, a species of Lovage. An old woman had been in the frequent custom of drinking a tincture of the latter plant as a cordial and remedy for pectoral ailments, and usually prepared it herself. One day her complaint being more troublesome than usual, she took an ounce before supper, another after it, and half an ounce more at midnight. She died in the course of the night. Not long afterwards three individuals in the family drank each of them an ounce of the old woman's cordial which she left behind her. They were all taken ill in the course of half an hour. One of them had inexpressible anguish, and sense of burning in the throat and stomach, vomiting, purging, tenderness of the epigastrium and colic, afterwards delirium, manifesting itself in loud cries and violent running; but emetics and emollient drinks calmed his sufferings, and in two days he recovered. Another man of weaker habit of body began to stagger, and appeared intoxicated, then was seized with violent vomiting, purging, and acute colic pains; and he died in two hours. The third, a young female, complained of a sense of burning and enlargement of the tongue, and then of burning along the gullet down to the abdomen; she was soon after attacked with shivering, swelling of the face, vomiting, purging, and violent colic, and she died in a state of great agitation two hours and a half after drinking the cordial. The bodies were examined, and the only appearance of note was great redness of the inner membrane of the stomach and small intestines. A medico-legal examination having been ordered, M. Degland, physician at Lille, discovered that the tincture had been made of the fresh roots of the *Aconitum napellus*.

These cases illustrate excellently the double, narcotico-acrid properties of this poison, as well as the extreme energy of the root in its fresh state.

Test of the Soundness of the Lungs, (from a Letter to one of the Editors, by Dr Lyons, Staff-Surgeon, Edinburgh.)—DEAR SIR,—The plan which I principally confide in for the purpose of ascertaining the integrity of the lungs of recruits, since the commencement of this year, continues to answer my most sanguine expectations; and as it appears to meet your approbation, as well as that of many of my Edinburgh medical friends, I beg that you will obtain for it a place in the Medical Journal, in order that it may become more extensively known.

Most officers placed in a situation similar to myself will speedily recognize in it a much more applicable assistant in difficult chest cases than the stethoscope; for by the simple process in which this plan consists they can ascertain at once whether the respiratory organ labours under any diminution of its capacity by the formation of vomicae, or from the deposition or infiltration of tubercular matter, coagulable lymph, hepatization, or matter between the pleura costalis and pulmonalis; it likewise detects any morbid irritability of the bronchial tubes which gives rise to dyspnoea. When the deaths from phthisis among the troops, and the numerous cases of young and well-looking phthisical subjects, who daily present themselves for admission into the service are considered, and the difficulties which the most experienced medical officers often meet with in ascertaining precisely the state of their lungs, it must be allowed that any plan which would obviate the difficulty would prove highly useful to the military and naval service. Most of my professional friends to whom I have mentioned the plan are of opinion, that, in exploring the chest in the incipient states of tubercular phthisis, and in pleuritis with effusion, it cannot fail to prove of great utility to medical men. In the latter of these diseases it readily detects the progress of absorption of the fluid, and is not liable to the objections which are raised against the stethoscope, on the grounds of delicacy, or exposure to the surrounding atmosphere. In many instances the diagnoses formed from this plan were corroborated by the stethoscope, percussion, or immediate auscultation, as well as by the confession of the individuals, after their hopes of entering the service were destroyed. The patient, after being examined for external causes of disqualification, is desired to inspire fully, and during the following expiration to count as far as he can slowly and audibly, without allowing himself to take another inspiration. The number of seconds he is enabled to continue counting in the expiration is noted by a watch with a second hand: this time will be found to increase in proportion to the integrity of the lungs. In confirmed phthisis the time of counting never exceeds eight, frequently falls under six or seven seconds; in pleuritis and pneumonia it seldom lasts for more than nine, and frequently falls as low as six and four; in hepatization it ranges in proportion to the extent of disease from eight to fourteen. In a young man, strong-looking and healthy, discovered to have dyspnoea, with a slight tinge of lip and ear, and also in an asthmatic patient during the absence of the fit, although a healthy-looking young man, it did not exceed ten. In a perfect inspiration, with apparently sound lungs, the individual counts from twenty-five to thirty seconds during the expiration. After the most perfect inspiration the time of counting during the expiration does not exceed thirty-five seconds.

A very few trials will enable the most inexperienced persons to apply this plan, and to make the necessary allowances in particular cases.—I am, my Dear Sir, truly yours,

WILLIAM LYONS.

Medical Graduations at Edinburgh.—On the 1st of August 1827, the Senatus Academicus of the University of Edinburgh conferred the degree of Doctor in Medicine on the following Gentlemen, in number one hundred and sixty, after having gone through the appointed examinations, and defended publicly their inaugural dissertations.

Of Scotland.

Charles Alison, . . .	De Ophthalmia.	Robert Wardlaw Bruce, De Pneumonia.
Robert Arbuckle, . .	— Rheumatismo Acuto.	James Bruce, . . . — Prostatæ Tumida.
Alexander Barron, . .	— Contagio.	James Burgess, . . . — Hydrothorace.
Robert Bell, ...	— Rubeola.	Alexander Burn, . . . — Entozois.
William Brown, . . .	— Rheumatismo Acuto.	Matthew Chalmers, . . — Podagra.
		John Coldstream, . . . — Indole Morborum Periodica.

William S. Cowan,	De Rubcola.	Willoughby Arding,	De Temperamen-
Joseph Crichton	— Febre.		to Nervoso.
John Cruickshank,	— Retinæ Fabri-	John Tatam Banks,	— Typho.
	ca et Usu.	James Barry,	— Erysipelate.
William F. Cumming,	— Aortæ Aneur-	John Basset,	— Aneurismate.
	rismate.	Charles Bell,	— Dyspepsia.
James C. Cumming,	— Anasarca.	Adam Chadwick,	— Inflamma-
A. Cunninghame,	— Pulsus Varie-		tione.
	tatibus.	William D. Chowne,	— Febre Puer-
William M. Dalgleish,	— Aneurismate.		peratum.
Robert Douglas,	— Iodino.	John Cockcroft,	— Purpura.
James Duncan,	— Cholera Spas-	William D. Cookson	— Epilepsia.
	modica.	William H. Cottle,	— Dyspepsia.
Robert Elliot,	— Hydrothorace.	Thomas Arthur Cotton,	— Febre Hecti-
Robert Fullarton,	— Diabete.		ca.
Charles Fyfe,	— Injuriis Capi-	William Crellin,	— Phthisi.
	tis	Edward Denis De Vitre,	— Affusione Fri-
William Gordon,	— Apoplexia.		gida tanquam Febrium Remedio.
W. B. C. Graham,	— Usu Medico	James Durie,	— Homine.
	Iodini.	Birt Dynely Edmonds,	— Dyspepsia.
C. B. Handyside,	— Necrosi.	John Epps,	— Insania.
John Inglis,	— Cholera Indiæ	Charles Fox Favell,	— Phlegmasia.
	Epidemica.	George Fyfe,	— Aneurismate.
George Irving,	— Ciborum Qua-	William Fleming,	— Nexu Corporis
	litatibus.		cum Animo.
Archibald Kean,	— Concoctione.	Henry Gillet,	— Dyspepsia.
William Laing,	— Apoplexia.	Frederick B. Glasspoole,	— Febris Epide-
Walter Laurie,	— Hepatitide		miciæ Natura Contagiosa.
Charles P. Livingstone,	— Cholera Epi-	Stephen Hall,	— Scarlatina.
	demica.	Thomas Haworth,	— Concussu Ce-
John Loudon,	— Dysenteria.		rebri et Extravasatione.
Parquhar M'Crae,	— Morbis Auris.	William Charles Henry,	— Tuberculorum
James M. M'Culloch,	— Sympathia		Origine.
	Genitali.	Charles Hingston,	— Motu Sangu-
David M. M'Gibbon,	— Hepatitide		nis.
	Acuta.	John Hogg,	— Phthisi.
John Macrobin,	— Peripneumo-	George C. Holland,	— Aneurismate
	nia.		Cordis.
John Moffat,	— Emphyse-	Fewster Robert Horner,	— Scrofula.
	mate.	Richard Jones,	— Febre et In-
John Munro,	— Dyspepsia.		flammatione.
James Petrie,	— Syphilide con-	William Kay,	— Typho.
	genita.	James Phillips Kay,	— Motu Muscu-
Robert B. Richmond,	— Febre.		lari.
David Russell,	— Angina Pec-	William King,	— Functionibus
	toris.		et Morbis quinti paris Nervorum.
Henry B. Shairp,	— Cynanche	Nathaniel Lister,	— Glandulis Sa-
	Tracheali.		livaribus atque Saliva.
James Sidey,	— Pneumonia.	James Bradley Mellor	— Curvatura
Alexander Stephen,	— Apoplexia.		Spinæ.
William Thomson,	— Dracuncula,	Edward Moore,	— Vino.
	sive Filaria Medinensi.	Pearce Rogers Nesbitt,	— Vasis Sangu-
Thomas Thomson,	— Catameniiis.		nen vehementibus.
David Staig Thorburn,	— Hepatitide.	W. Thomas M. Pattison,	— Rheumatismo.
Adam Warden,	— Dysenteria	John Penkivil,	— Apoplexia.
William White,	— Scrofula.	Alfred Pett,	— Iritide.
Moses White,	— Febre Flava	William F. Rawdon,	— Erysipelate.
Charles Wilson,	— Pathologia	Jacob Ridely,	— Absorptione.
	Bilis.	William Rushton,	— Inflamma-
John A. Wright,	— Cholera.		tione.
Thomas Young,	— Oculo.	John Savage,	— Rheumatismo
	<i>From England.</i>		Acuto et Chronico.
John Alexander,	— Phthisi Pul-	Edward Scholfield,	— Morbis Hære-
	monali.		ditariis vel de Transmissione Morbi.

Charles H. Stone, . . .	De Syncho.	John Nugent, . . .	De Ascite.
Matthew Storm, . . .	— Inflammatione Membranarum Synovialium.	William H. O'Connell, —	Hepatitide.
James Stuart, . . .	— Concoctione.	Charles O'Connor, . . .	— Hepatitide.
John Taylor, . . .	— Apoplexia.	Charles O'Reilly, . . .	— Dysenteria.
John Twynan, . . .	— Insania.	Peter Perrin, . . .	— Angina Pectoris.
William Wilson, . . .	— Dysenteria.	John H. Sealy, . . .	— Imaginationia Potestate in Corporis Morbis.
William Wood, . . .	— Morbis Mamarum.	Robert Shannon, . . .	— Ascite.
<i>From Wales.</i>		John Speer, . . .	— Dysenteria.
Richard Rowland, . . .	— Apoplexia.	C. Dawson Stewart, —	Strictura Urethrae.
Henry William, . . .	— Sanguinis Circulatione	Robert Stewart, . . .	— Erysipelate.
<i>From Ireland.</i>		Henry Taaffe, . . .	— Pneumonia.
Thomas M. Ahearne, —	Actione Hydrargyri, &c.	John Thwaites, . . .	— Injuriis Capitis.
Joseph Clarke, . . .	— Apoplexia.	<i>From Guernsey.</i>	
Peter Conolan, . . .	— Dyspepsia.	Charles James Stephens, —	Enteritide.
Edward Delany, . . .	— Rheumatismo.	<i>From Barbadoes</i>	
James Davenport, . . .	— Scarlatina.	Henry Bishop, . . .	— Absorbentibus.
George Davis, . . .	— Effusione Serosa, cum observationibus paucis de Venarum Pulsatione.	<i>From Jamaica.</i>	
Edmund Dowell, . . .	— Hypochondriasi.	George Archer, . . .	— Hydrophobia.
R. Martin Forsayeth, —	Ophthalmia.	John Edward Chambers, —	Ictero.
Richard T. Fowler, . . .	— Pneumonia.	John Clark, . . .	— Usu Hydrargyri.
Charles Y. Haines, . . .	— Scarlatina.	William C. Smith, . . .	— Concoctione.
Thomas Haire, . . .	— Hydrocephalo Acuto.	<i>From Canada.</i>	
W. Richard Hamilton, —	Enteritide.	Francis C. T. Arnoldi, —	Asphyxia.
Walter Harris, . . .	— Enteritide.	Francis William Porter, —	Cynanche Tracheali.
Travers Homan, . . .	— Ophthalmia.	<i>From America.</i>	
Evory Kennedy, . . .	— Febre Puerperarum.	William S. Scott, . . .	— Tuberculis.
Peter Kinin, . . .	— Chorea.	<i>From Spain.</i>	
Thomas Kirkpatrick, —	Dysenteria.	Marian Battles, . . .	— Mania.
William Lyons, . . .	— Delirio Trementi.	<i>From St Petersburg.</i>	
George M'Dermott . . .	— Dysenteria.	John Drury, . . .	— Emansione Mensium.
John Smylie M'Dowell, —	Phthisi Tuberculari.	<i>From Rio Janeiro.</i>	
David M'Manas, . . .	— Asthmate.	Edward Williams, . . .	— Quibusdam Phthiseosis Causis.
Patrick Moran, . . .	— Pneumonia.	<i>From Newfoundland.</i>	
Timothy Morressy, . . .	— Enteritide.	Thomas B. Reed, . . .	— Phrenitide.
Patrick Murphy, . . .	— Enteritide.	<i>From Trinidad.</i>	
J. Fitzgerald Murray, —	Visu.	St Louis Philip, . . .	— Tetano.
		<i>From Bahama.</i>	
		Henry Joseph Hunt, —	Chorea.

Surgical Examinations at Edinburgh.—August 1, 1827. Since August 1826 the following Gentlemen, one hundred and ninety-nine in number, have appeared before the Royal College of Surgeons of Edinburgh, and having been found fully qualified to practise the arts of Anatomy, Surgery, and Pharmacy, received Diplomas accordingly.

<i>Of Scotland.</i>		
Robert Allan,	Adam Thomson Jackson,	Alexander Laing,
Robert Pridie,	John Beath,	Charles Morehead,
Thomas Henderson,	James Leathem Clarke,	Robert Primrose,
Alexander Cunningham,	James Kellie,	Robert Anderson,
Matthew Ross Deas,	Stephen Lawson,	William C. Robertson,
John Coldstream,	John Gardner,	James Bruce,
James Clephane Minto,	John Innes Allan Moir,	Thomas Wharton Jones,
Samuel M. Hadaway,	David Young,	Francis C. W. Cavins,
John Campbell Boswell,	John Inglis,	Peter Duncan,
	Charles Alison,	James Smith,

Duncan Campbell,
George Smith,
William Ford,
William Henry Arrott,
James Arrott,
Angus James Campbell,
Donald Sinclair,
Robert Fullarton,
Andrew Wilson,
Hugh Wood,
William Dalglish,
Thomas Steele,
James Forsyth,
Charles Crawford,
Peter Robb,
William Stewart,
William McGowan,
Thomas Thomson Smith,
Francis Shortt Arnott,
Archibald Arnott,
James Burgess,
James Henderson,
James Bell Jardine,
William Ogle Bell Irving,
A. Yorstoun Anderson,
Thomas Smith,
James Rae,
John Armstrong Wright,
John Cowan,
Laurence Davidson,
Robert Bell,
David Ure,
John Wardrop Moore,
William Middlemas,
James Grant Seton,
Thomas Hislop,
William Robb,
Patrick Stuart,
T. Thomson Dempster,
James Mill,
David Deas,
James Brabner,
Robert Scott,
Robert Douglas,
John Chalmers,
Alexander Sanderson,
R. Chalmers Knight,
William Binny Webster,
David Sturrock,
John Maitland,
Daniel MacLachlan,
Thomas Galbraith Logan,
Lachlan Grant,
William Welsh Forbes,
Alexander Macdonell,
James Macdonald,
John Cruickshank,
Richard Mein,
Charles Croll,

George Coutts,
James Napier,
J. Archibald Fullarton,
George Gibson,
Farquhar McCrae,
John Mitchell,
Alexander Anderson,
David Russell,
Alexander Barron,
Æneas Mackintosh,
John Drever,
David Strang,
Robert Omond,
James Mason,
William Nicol Aitken,
William Bayne,
William M^cIsaac,
John Gibson,
James Proudfoot,
Fraser Thomson,
Samuel Thomas Deseret,
John M^cKay,
David Lowe,
William Dow,
Robert George Bruce,
Duncan Menzies,
Robert Ballantine,
Peter Strang,
Andrew Halliday Hall,
William Bowie,
Malcolm M^cNiell Rind,
John Ross,
John Stuart,
Robert Arbuckle,
William Laing,

From England.

John Knapp,
John Nairn,
William Smith,
Llewelyn Jones,
John Hogg,
William Park,
W. Margaret Dalglish,
John Davie Bassett,
Pearce Rogers Nisbett,
Lenox T. Cunningham,
William Kay,
Thomas Haworth,
Richard Roscoe,
Frederick Horatio Fisher,
Charles Robert Butter,
Patrick Mollison,
J. Atkinson Ransome,
William Ainsworth,
George William Chester,
Edward Alexander Smith,
Thomas Annandale,
George Fife,

Fred. Bream Glasspoole,
Arthur Hedley,
John Leithead,
Robert Melville Wilson,
William Burrell,
Henry Robert Chaplin,
William Cartmel,
Barnard James Boulton,
Aug. Fred. A. Greeves,
Edward Scholfield,
Thomas Skardon Taylor.

From Isle of Man.

Thomas Murray Teare.

From Ireland.

John Young Skelton,
John Sloan,
John Tully,
Robert Shannon,
Robert Murray,
Hugh Neill,
Gregor M^cGregor,
William Barry,
Julius Besnard Evans,
Henry Augustus Cæsar,
Edward Delany,
Joseph Clarke,
Daniel Byrne,
Edward Stack,
Samuel Lewis Potter,
Anthony Patrick Mahon,
Michael Moore,
John Murtagh,
James Shields,
William John Trotter,
Charles O'Connor,
Edward Denniston,
Charles Devlan,
Thomas Scully,
James Snodgrass,
John King,
Richard John Walsh,
James M^cCulloch,
Thomas Burrows.

From the Colonies.

Robert Farquhar Osborn,
George Waddell,
Isidore Berthelot,
John Jenkin Maclean,
George Archer,
James Campbell,
John Clarke,
Alexander Wight,
Alexander Robertson,
William Matthews,
Henry Joseph Hunt,
Thomas Boucher Reed.

From St Petersburg.

Robert Auld.

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